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On Changes in the Recorded Mortality from Cancer and their Possible Interpretation

BY

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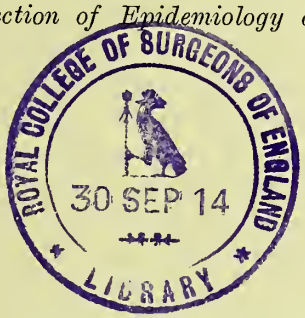
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On Changes in the Recorded Mortality from Cancer and their Possible Interpretation.

By Major Greenwood, jun., and Frances Wood.[1]

The questions considered in this paper have aroused the interest of many inquirers and are, indeed, well calculated to do so. If it could be shown that cancer is no more prevalent now than it has been at any time in the past, attempts to discern a causal relation between the progress of the disease and such changes in our habits of life as, for instance, alterations of diet or some hypothetical increase in the wear and tear of daily life, must be waste of labour. Alternatively, if there be truth in the popular view that cancer is on the increase it must be of importance to determine the limits within which such increment may be presumed to lie.

Writers of repute have arrived at contradictory conclusions respecting the matter, and the task we set ourselves was an examination of such statistical evidence as has been adduced on both sides of the controversy and an attempt to pursue further inquiries which might be suggested by the works examined. We have not been led to advocate any strikingly novel opinions, but we may hope that our remarks will serve to initiate a discussion and perhaps suggest to others fruitful lines of research. We have made no attempt to furnish a complete critical review of the literature; to do so would exceed the limits we have assigned to this essay, and an analysis of certain typical and important papers will be sufficient for our purpose.

[1] From the Statistical Laboratory of the Lister Institute of Preventive Medicine.

In various issues of the Registrar-General's Annual Reports during the eighties doubts were expressed as to how far the *recorded* increase of cancer mortality could be regarded as evidence of a real extension of the disease—e.g., in the forty-fifth and forty-sixth Annual Reports—but, so far as we know, the first writers to publish a detailed study of the matter were Mr. King and Dr. Newsholme, whose paper appeared in 1893. In the first part of that paper a comparison was instituted between the course of cancer mortality from 1860 to 1890 in England, Scotland, Ireland (males and females separated in each case) and the experience of the Scottish Widows' Fund.[1] Standardized rates were computed and curves applied by Mr. King's modification of Milne's graphic process. The characteristics of the resulting curves were as follows: The Irish males and females exhibit the lowest rates, which run approximately parallel and are not widely separated vertically. The English curves are far apart and show an increase in gradient with time, especially perhaps that of the males. The Scottish curves are similar but the vertical distance between them is much less than in the last mentioned case. The Scottish Widows' Fund curve (which we suppose refers almost entirely to male lives) corresponds in absolute height above the axis to the Scottish males, but its gradient, although positive, is decidedly less steep. The authors attributed the low rates for Ireland to poor diagnosis, the easier gradient of the Scottish Widows' Fund to a higher level of skill among the medical men attending persons of the insured class and the nearer approximation of male and female rates in Scotland as compared with England to better education of the general practitioner in the former country. In the concluding part of the paper the experience of Frankfurt-on-Main is described, a city in which cancer deaths had long been classified in accordance with the part of the body primarily affected. The sites were divided into accessible and inaccessible, rates of mortality were formed and the results compared. The authors concluded that the "one result of surpassing importance to be derived from them is that *in those parts of the body in which cancer is easily accessible and detected there has been no increase in the mortality from it between* 1860 *and* 1889."

This paper was adversely criticized by W. Roger Williams, who objected to some details of the classification of sites and quoted in

[1] It must be remarked that the total number of deaths recorded in the last-mentioned experience was only 545, while the smallest number in any single septennial period of the other series was 2,501. In common with other observers we have found it necessary to be very cautious in the use of rates unless deduced from very large total numbers of observations.

rebuttal of the authors' general results certain hospital data. We agree with Newsholme and King in thinking that the comparison attempted to be made was illegitimate and are unable to accept the inferences drawn by Williams. There are, however, some points in King and Newsholme's paper which invite further examination. Much importance was attached to the contrast between the experience of the Scottish Widows' Fund and that of the general population. The numbers available in the former case were small and the method of smoothing, which involved a transfer of seven deaths from the second to the first septennium — i.e., increased the total in the latter by 16·7 per cent. of its original value—may have helped to diminish the gradient of the curve. But, apart from such questions of method, the nature of the comparison instituted seems questionable. The insured lives represented a highly select class from the economic aspect; their sole distinction could hardly reside in the fact, or supposed fact, that they received more skilled medical attendance than the general population. It is assumed that the difference between the trends of the curves is entirely accounted for by such differences in diagnostic skill, an assumption which involves the further hypothesis that the true occupational distribution of cancer must be constant from year to year. Both assumptions may be correct, neither is self-evidently true and no evidence is tendered in support of them.

We must also insist on the fact that no precise meaning has been assigned to the phrase, "improved diagnosis"; so far as we know, Dr. E. F. Bashford was the first inquirer to give it an intelligible meaning in connexion with cancer. We do not think that the progress of research has placed in the hands of the ordinary practitioner direct means of diagnosing cancer not available even so long as fifty years ago. The one method which, as the evidence collected by the workers of the Imperial Cancer Fund indicates, does increase the recorded incidence of cancer is post-mortem examination, especially when accompanied by the use of the microscope. We do not know of any evidence suggesting that general practitioners carry out such examinations more frequently even now than in the past. No changes in the sentiments of the laity have tended to make such procedures less repugnant to surviving relatives, and we imagine that the wishes of his clients must still have weight with the private practitioner. There are, of course, many reasons for thinking that progressively more deaths occur in institutions where autopsies are performed, but even now the proportion of deaths so occurring is not, except in London, very large, nor

would it affect the customers of insurance companies, the vast majority of whom are attended by private practitioners. It would seem, therefore, that the contrast between the experience of the Scottish Widows' Fund and that of the general population is not very conclusive evidence that improved diagnosis accounts for the recorded increase of cancer mortality. There is also some inconsistency in attaching importance to the approximation of male and female rates in Scotland as evidence of good diagnosis in that country, when the approximation, which is still closer in Ireland, is not allowed to mitigate the sentence of inferiority pronounced upon its medical condition.

Turning to the Frankfurt data, we do not very well see how the figures published really support the conclusions stated to be drawn from them. Certainly the male rates for accessible sites are irregular, and being derived from small absolute numbers do not provide a basis for any deduction ; but the inaccessible rates are also irregular, although based upon larger numbers. The figures for women are puzzling ; the inaccessible rates show a fairly regular increase, but so do the rates for accessible sites. If we compare the last figure with the first, in the "inaccessible" group, the 1888-89 rate is about 146 per cent. of the 1860-66 rate, and the corresponding " accessible " figure is 123 per cent., but the regularity of change in the latter instance is marred by a sudden drop in 1881-87 followed by a large increase in 1888-89. Perhaps this is due to paucity of numbers, but it can hardly be contended that there is *no* evidence of increase.

Some results detailed below support King and Newsholme's assertion that males and females suffer equally from inaccessible cancer, but the numbers quoted on p. 227 of their paper are not very good evidence. They seem to have been obtained by averaging the rates set out in Table XVI, p. 242, and little importance can be attached to simple averages derived from so short and irregular a series, even were each rate calculated from an equal number of years, which was not the case (the fifth entry is deduced from the experience of one year, the others from quinquennia). King and Newsholme's paper marked an epoch in the statistical study of the problem; it indicated an important source of fallacy and suggested ways of appreciating the magnitude of the error involved, but the conclusions summarized in it were not, we think, entirely borne out by the evidence tendered.

In the years following the publication of King and Newsholme's paper authors were generally disposed to pursue the lines of inquiry suggested by their work. Thus, in 1902, de Bovis, from a study of

the cancer rates of Frankfurt, Vienna, Hamburg, Berlin, Christiania and Switzerland, thought that the change in cancer death-rates was due to an increased mortality from visceral cancer, and he drew attention to the decrease of deaths from unknown causes reported in Switzerland coincidently with a rise in the cancer rate. In 1903 Robertson pointed out the importance of correcting for institutional deaths, a frequent cause of error, although he does not seem to have applied age-corrections to his data. Templeman's study of the Dundee statistics also showed a greater relative increase of the inaccessible sites, but the data (Dundee, 1877-1901) were not very numerous. This author held that, in spite of the facts mentioned, there had been some real increase of cancer. Another paper bearing on the question of diagnosis was that of Heimann who, on the strength of international comparisons, argued that the increase of cancer had been relatively smallest in those countries in which death certification is presumably most accurate. In 1904, Dr. Lazarus Barlow and Dr. Taylor published an elaborate paper on the experience of the Middlesex and St. George's Hospitals. From the recorded ages of hospital patients "populations" were formed of persons who attained the age of 35 in different years, and had either died in hospital or were at the time of record suffering from definitely mortal diseases—e.g., thoracic or abdominal aneurysm. The causes of death were classified as "cancer" or "not cancer," and the percentage of the total borne by the former group in each year's "population" was computed. Since such "populations" could not be complete for the more recent years, the figures for the latter were corrected on the assumption that the proportional distribution obtaining in 1895, 1896 and 1897 would apply in future populations. The results showed an increase in the proportional cancer mortality of both sexes up to 1870; after that time the percentage mortality of women remained stationary, while that of men continued to increase. Somewhat similar results were yielded by both the hospitals studied, and the authors held that the real incidence of cancer must have increased. This judgment was more recently emphasized by one of the authors, Dr. Lazarus Barlow, who in a retrospect appearing in vol. xxiii of the *Archives of the Middlesex Hospital*, writes: "The experience of the Middlesex Hospital was that cancer has been increasing steadily since the beginning of the nineteenth century in males, and was still increasing, but that in females it increased up to about the year 1874, and since that time has maintained its high level unaltered." The authors enumerated various sources of fallacy to which their method might

be subject, but we shall only deal with one which is, we think, of special importance. A possible interpretation of their results would be that, while the true incidence of cancer was constant, the relative hospital mortality had increased owing to (1) a greater proportion of admissions from cancer, (2) a lower mortality from causes other than cancer, or (3) *a different selection of hospital cases.* The first objection they hold to be met by the fact that the ratio of cancer cases to all admissions has not increased sensibly during the period 1870-99 (the figures do, however, as the authors pointed out, show some tendency to increase); they also argue that the mortality from causes other than cancer in persons aged over 35 has not diminished greatly, so that the increasing ratio could not be explained in this way. Neither of these contentions meets the difficulty italicized. It is certain that a hospital population is not a random sample of the general population, but both pathologically and economically select. If we first suppose that the basis of this selection has not been changed, that all who present themselves for admission are, so far as space permits, admitted and are of the same economic class, the increased relative death-rate from cancer would only be evidence that cancer is more fatal among persons of the class furnishing hospital patients, and only then if the death-rate from other causes *in that class* has been constant. We think, as a matter of fact, that neither the economic class nor the general fatality rate observed among hospital patients can be assumed to have remained constant, although in the case of certain occupations and diseases Greenwood and Candy failed to demonstrate any very striking change during the last fifty years (they found that the pneumonia fatality rate had not markedly diminished since 1854, and that the proportion of labourers among the hospital patients diminished between 1783 and 1907, but that there were difficulties in accepting the comparison as valid owing to possible differences of classification).

Another difficulty is this: if the policy of the hospital has not changed and the same class of patients presents itself as formerly, we should expect to find a larger proportion of *admissions* for cancer if the incidence of that disease upon the class furnishing hospital patients has increased; but the percentage admissions for cancer to the Middlesex Hospital has remained (very approximately) constant, and therefore, as it seems to us, if cancer has increased among the population the policy of the hospital or, what comes to the same thing, the policy of those who resort to the hospital cannot have remained

constant, although we can apply no direct test to either of these conclusions.

For these reasons it does not appear to us that Dr. Lazarus Barlow and Dr. Taylor's work, interesting as it is in other respects, really illuminates the problem with which we are here concerned.

In 1907, F. Prinzing published an essay in which he pointed out that in different districts with varying total cancer rates, a high rate seemed to depend upon cancer of the stomach and œsophagus, the rates for other organs remaining fairly constant. In his treatise on medical statistics published in the same year, this author points out certain pitfalls, but nevertheless remarks: "Aus den genannten Gründen darf man diese Ziffern nicht als einwandfreien Beweis der Zunahme der Krebssterblichkeit betrachten; trotzdem werden wir sie heute nicht mehr bezweifeln können" (op. cit., p. 400). We cannot, however, say that Prinzing's article contains any justification of the confidence expressed in the latter part of the quotation.

In a paper in *Public Health*, 1910-11, Clements argued that the increased recorded rate could not be entirely due to improved diagnosis because: (1) The final stages of the disease present a definite clinical picture; (2) more frequent microscopic examination would result in increasing the number of benign and reducing that of malignant tumours; (3) taking occupational groups, the higher the social status the lower the cancer-rate, although better medical skill would be available; (4) medical practitioners are not now in a better position to diagnose cancer than they were thirty years ago. Of these statements, (1) is too sweeping and (2) is not in accordance with the results published by Bashford and Murray. As to (3), some results obtained in the statistical laboratory of the Lister Institute by Brown and Lal suggest that there is no positive and perhaps even a negative correlation between occupational status and cancer mortality, but whether we can really assert that diagnosis is more accurate among the wealthier classes is doubtful in the case of large cities, where the poor resort to hospitals in considerable numbers. We have already expressed our opinion that (4) is a true statement. Among other recent authors who support the view that cancer has increased in frequency are Sanes and Parkes. The former based his conclusions upon a study of the proportions of cancers of the skin and cancers of the breast among all forms, the data being derived from the registration area of the United States, for the years 1900-07; the latter dealt with some of the English statistics, to which we shall have to refer below. With regard to Sanes's work we

think that his data were not sufficiently detailed to be of first-rate importance, quite apart from the general criticisms of the American cancer statistics, which have recently been put forth by Bashford.

In the Seventy-fourth Annual Report of the Registrar-General, Dr. Stevenson makes an important contribution to the literature of our subject. He first points out that the corrected or, to give it its new name, the standardized cancer rate of males is much higher in London than in any of the other administrative groups reviewed; the rate of female mortality is not, however, excessive. He suggests that this difference may be explained on the supposition that cancer is more easily diagnosed in females than in males, and that the London statistics more nearly represent the true incidence of the disease than do other data. There are two reasons for this belief: (1) A considerably higher percentage of cancer deaths in London is returned from institutions, in some of which post-mortem examinations are regularly performed, than elsewhere; (2) the accuracy of certification, the criterion being the proportional frequency of deaths assigned to indefinite causes, is greatest in London. Dr. Stevenson also points out that the male rate has been steadily overtaking the female rate in London: in 1891-95 the former was only two-thirds of the latter, now they are equal. In the same way he thinks it might be possible to explain the peculiar history of the increase, paying attention to age and sex. Thus, while the records show a continuous advance in all age-groups among males, change was arrested at ages 35 to 45 among females some twenty years ago, and ceased in the group 45 to 55 later. The suggestion is that we might explain the former result by supposing that there is still, taking the country as a whole, room for improvement in diagnosis at every age in males; while the cessation of increase in the rate among middle-aged women may mean that cancer being better diagnosed at the earlier ages is now seldom overlooked, although it was so formerly. Dr. Stevenson thinks that a table showing the rates at ages 45 to 65, and at 65 and over, in different districts, generally supports this contention, although he notes that the rates for extra-metropolitan areas when compared among themselves do not seem quite favourable to it. We must also refer to a comparison between the distributions of deaths classified by sites as reported from different classes of institution and from private practice. This does not bear upon the main problem, but is indirectly of much importance, since it shows that the type of the institutional cancer "population" differs from that of the non-hospital class, a result which strengthens our belief that hospital statistics

must be interpreted with caution by students of the problem under discussion.

A review of the whole of the evidence, which we have of course been obliged to summarize imperfectly, leads Dr. Stevenson to doubt whether the real incidence of cancer in England and Wales has increased, and the hypothesis he puts forward is enunciated in the following question: "Whether England and Wales in 1911 do not compare with England and Wales in 1881 more or less as London in 1911 does with rural districts in 1911?"[1] That is to say, the change is the product of continuously improving diagnosis—the capital leads the way, and the provinces now occupy ground formerly occupied by London. It seems to us that Dr. Stevenson's report is the most valuable contribution to the statistical branch of the subject which has yet appeared. Not only is it based upon a considerable body of evidence, but it gives the hypothesis used by the earlier writers, such as King and Newsholme, a more intelligible form. We know that post-mortem investigation is an important means of assuring accuracy of diagnosis, and we know that such inquiries are more frequently carried out in London; we also know that the improved pathological equipment of the London schools both by precept and example reacts upon the provinces. The mind can therefore readily conceive how the change premised by Dr. Stevenson might have been brought about, and his inquiry forms a fitting starting point for further investigation. We shall describe our own results lower down, here we confine ourselves to a few reflections prompted by his own remarks.

In the first place, as he observes, the order of the other districts, apart from London, is irregular. Thus, taking males 45 to 65, in Wales the county boroughs have the lowest rate, other urban districts and rural districts being practically equal. Again the Midland county boroughs have the lowest rate for males over 65. In the north the county boroughs have a lower rate than other urban districts. Among females the orders are very irregular, a statement which is still true when the capital is included.

In the second place, the question of female mortality needs further consideration. If we classify the 20,313 deaths from cancer among females in 1911, according to the scheme given on p. 18 of the second

[1] The wording of this paragraph has tended to convey the impression that this question represents a settled opinion. We desire explicitly to state that we understand Dr. Stevenson's question to be a purely hypothetical one, and not to convey any definite conclusion.—M. G., F. W.

report of the Imperial Cancer Research Fund, into accessible, inaccessible and intermediate sites, we find that 41 per cent. were accessible, 48·2 per cent. inaccessible, 6·5 per cent. intermediate, and 4·3 per cent. could not be classified. If we assume that improvements in diagnosis would principally affect the inaccessible group, there is a large field for the operation of this cause in the case of women. But the London figures do not, in fact, show a generally higher rate than other districts. This really understates the difficulty, and for the following reason. The statistics of Table I of Bashford and Murray's report already cited, admittedly based upon small numbers however, show that post-mortem examinations are likely to produce considerable additions to the numbers of recorded cancers of the uterus. Thus, if we have correctly interpreted the table, out of forty-two cases of carcinoma eleven were only recognized post mortem or after operation. Confining ourselves to post-mortem cases, out of forty, nine (23 per cent.) were added by such examination. Now compare this with the statistics of a typically inaccessible site, the stomach. In 173 records thirty-nine (23 per cent.) were due to the autopsy; the proportional additions were the same in the two cases. It is difficult to understand why London does not exhibit a less marked but still substantial superiority over the provinces in the case of females also.

A third point is that Dr. Stevenson's argument seems to assume that the diagnosis of cancer in women between 35 and 45 ceased to improve as long as twenty years ago. There is evidence in favour of this—viz., the observation that the proportion of accessible cancers is higher at the earlier ages, but we know of nothing else save the fact that the cancer rate *has* been steady at this age, which can be said to support the hypothesis, and the last-mentioned fact is, of course, not inconsistent with other views.

Let us now endeavour to test the hypothesis that the recorded changes are merely the reflections of improving diagnosis. It seemed to us that a comparison of changes in the rates of different parts of the country during a series of years might be helpful for the following reasons. If we compare London with (*a*) a mainly rural county or counties, (*b*) with an urban county, we should have to consider the following possibilities: (I) If at the beginning of the series of years the conditions for establishing a diagnosis of cancer were already on a much higher level in London than elsewhere, then throughout the series the other districts would tend to increase their rates faster than London *if* a steady general improvement in medical knowledge, or facilities for

effecting a diagnosis, were taking place. (2) If London not only started in a better position, but continued to improve at a faster rate, then the London annual curve should be the steeper. (3) If the course of events be quite different in different areas—e.g., the rates of change being variable—the gradients of the annual curves may bear any relation one to another. Such a comparison will, therefore, *not* provide us with a general solution, but it will throw light upon the simple and intelligible form of the hypothesis advanced by Dr. Stevenson. The point here raised seems to us one of great interest, and we may be permitted to explain it somewhat more fully.

Let us suppose that the incidence of some disease upon a community is strictly constant, that the same proportion of deaths from it really exists in each year, and that at the beginning of our observations no means of diagnosing it are available, so that no deaths are recorded. Now let us imagine that imperfect means of diagnosing the disease are introduced, and that they improve from year to year; the effect upon the curve of recorded rates will depend upon the way in which the improvements are effected. If, for instance, an infallible method be suddenly discovered and disseminated, the rate will shoot upwards, and then run parallel with the abscissa at a height corresponding or nearly corresponding to the real incidence. If smaller improvements be introduced at discontinuous intervals, the curve will exhibit a series of step-like ascensions until, as before, a constant level is ultimately reached. If the intervals be very short, the steps will be smoothed out and the constant level smoothly approached; in the most probable case the curve never becoming absolutely parallel with the base, but having for asymptote a straight line parallel to the axis of X (the abscissa), and intersecting the ordinate axis at a height measuring the true incidence. Given such a state of affairs, let us finally suppose that two fragments of the curve of rates are available for our inspection; a portion derived from the period when diagnosis was still very imperfect, and a portion from the later years of the experience when diagnosis had been further improved; let us say two periods of ten years in series, the last year of the first series being several years earlier than the first year of the second series. Had some revolutionary change come into operation in the course of one series or in the interval between the two, very startling differences might appear; thus, one of the curves might exhibit a sudden change of gradient, or, alternatively, the first might be ascending and the second be almost horizontal. But in the more probable case of small continuous improvements we should not expect any such dramatic result,

It would be likely that the two portions would not be long enough to give us reason to suppose that either was a fragment of a "curve," in the popular sense, at all. On the contrary, it would be more probable that each would, paying attention to chance fluctuations, seem to be as closely represented by a straight line as by any other locus. Indeed, in the case of such a perfectly continuous non-linear function as the logarithm of a number, we know that for short ranges a straight line very well represents the changes of the logarithm as the number is increased by constant amounts (a fact which justifies our customary method of nterpolating between tabular values of logarithms). But if the portions are taken from different parts of the curve, the representative straight ines will not have the same gradient; the nearer we are to perfection he less will be the slope. We illustrate the point in Diagram I, which

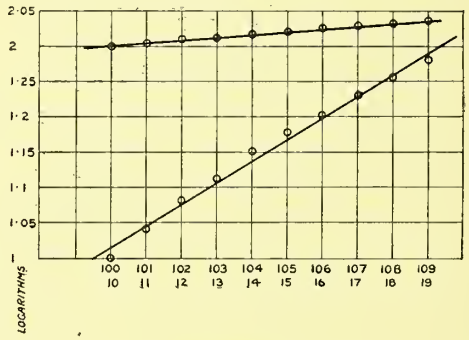


Diagram I.

The upper range of points represents the logarithms of 100—109. Fitted with a straight line. The lower range is a similar representation of logarithms 10—19.

portrays the line representing logs. 10 to 19 and that corresponding to logs. 100 to 109. Two conclusions emerge: the first, that in any short series of years we are not likely to detect the form of a curve which will ultimately run parallel with the abscissa; the second, that we should expect, assuming the hypothesis just described to be well founded, that straight lines would fairly represent such small portions, and that they would exhibit differences of gradient. Consequently, if we could regard different communities within a nation as being samples of the ideal curve at different parts of its journey, from the zero of complete non-recognition of the disease to the maximum of complete diagnosis, we should detect differences in gradient. This is the rationale of the method we devised to test Dr. Stevenson's form of

the hypothesis as applied to cancer, and although our particular application is subject to numerous imperfections, partly inherent in the data and partly the consequence of our being obliged to write this paper in a rather limited time, we believe the method to be serviceable other cases.

TABLE Ia.—CRUDE DEATH-RATE FROM CANCER PER 1,000,000 LIVING FOR THREE URBAN AN THREE RURAL REGISTRATION COUNTIES, 1881-1900. PERSONS.

Year	URBAN COUNTIES						RURAL COUNTIES					
	London		Lancashire		Northumberland		Lincolnshire		Somersetshire		Wiltshire	
	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*
1881	610	80	410	74	570	88	630	84	580	76	520	73
1882	630	82	410	74	520	80	610	81	610	80	630	89
1883	650	85	420	76	500	77	580	77	670	88	530	75
1884	650	85	430	78	470	73	590	78	660	86	590	83
1885	640	84	440	79	510	80	600	80	590	77	660	93
1886	648	85	457	83	580	90	603	80	703	92	598	84
1887	690	90	485	88	591	91	683	91	682	89	549	77
1888	683	89	512	92	626	97	652	87	752	99	652	92
1889	696	91	523	94	634	98	743	99	752	99	678	96
1890	786	103	555	100	660	102	767	102	769	101	801	113
1891	792	104	572	103	677	105	803	107	766	100	736	104
1892	759	99	582	105	691	107	745	99	771	101	717	101
1893	804	105	590	107	715	111	857	114	778	102	725	102
1894	810	106	595	107	685	106	816	108	795	104	895	126
1895	846	111	631	114	686	106	874	116	831	109	757	107
1896	870	114	647	117	733	113	903	120	832	109	792	112
1897	888	116	677	122	756	117	899	119	889	116	835	118
1898	918	120	703	127	754	117	837	111	887	116	726	102
1899	944	124	704	127	752	116	939	125	984	129	844	119
1900	964	126	730	132	812	126	933	124	962	126	938	132
Gradient of the best fitting straight line †	2·449		3·219		2·420		2·687		2·423		2·486	

* The mean of the whole series has been taken as a base.

† The straight lines have been fitted to the index numbers and not to the actual death-rates, in order that the gradients may be comparable one with another.

We decided to use London, the assumed high-water mark of diagnostic skill; Lancashire, a highly urbanized county; Northumberland, a partly urbanized county; and Wiltshire, Linconshire, and Somersetshire as instances of more or less rural areas. The periods 1881-1901 and 1901-11 are separately considered; in the former series we deal with persons, in the latter the sexes are taken separately. We begin with the 1881-1901 data. Diagram IIA shows the best fitting straight lines

applied (by the method of least squares) to index numbers formed by taking the mean rate of each series as 100, and Table Ia contains the data and gradients of the fitted lines. These constants and

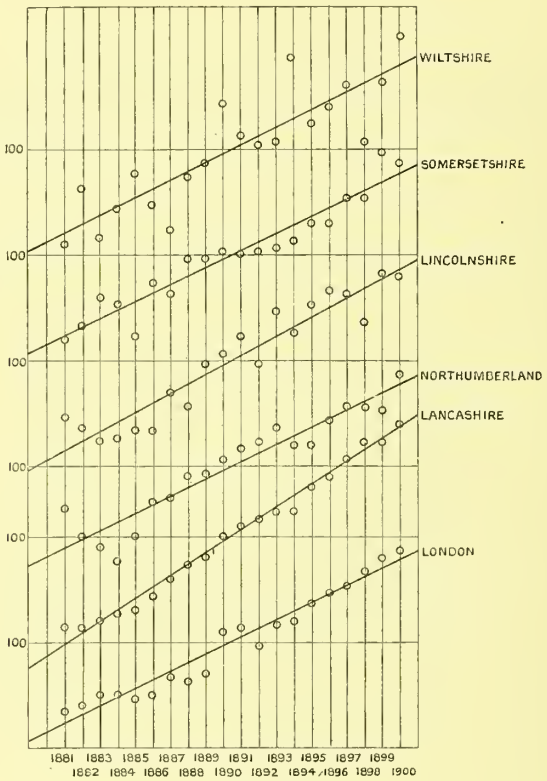


Diagram IIa.

Index numbers showing the changes in the crude death-rates from cancer for three urban and three rural registration counties with the best fitting straight lines,* 1881-1900. Persons.

* The tangents of the angles made by straight lines drawn upon this diagram with the abscissæ are not equal to the values given in the last line of Table I. This is because an arbitrary unit has been chosen in order to make a convenient diagram. Since the same unit has been used throughout the lines are perfectly comparable one with another. This applies to the diagrams throughout the paper, but in every case the unit chosen has been the same—viz., the base unit is five times as large as the vertical unit.

diagrams refer to index numbers computed from the crude rates. We next allowed for changes in age constitution by the following method, which, although not free from objection on theoretical grounds, has

the advantage of simplicity. The population of England and Wales in 1901 was taken as the standard population and the mean cancer rates at ages for 1901-11 as the standard rates. Age and sex correction factors were then computed for each county population (excepting Wiltshire, the results from which in the case of crude rates seemed to argue that the population at risk was too small to afford reliable indications) as shown at the censuses of 1881, 1891 and 1901. Correction factors for the intercensal years were prepared by adding to the factor

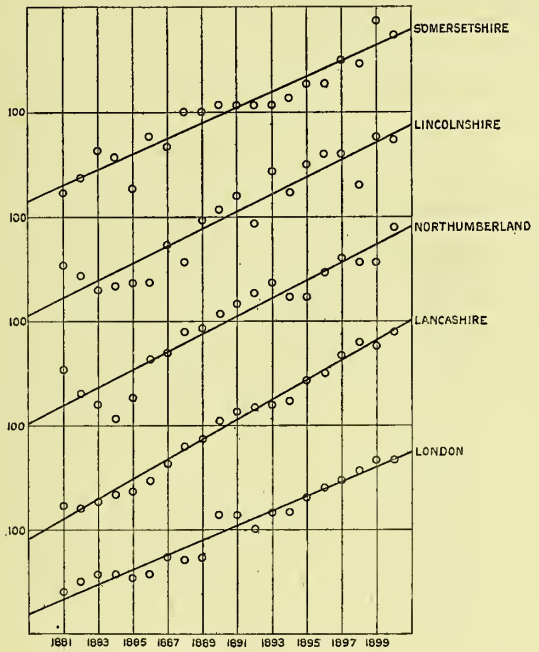


Diagram IIb.

Index numbers showing the changes in the corrected death-rates from cancer for three urban and two rural registration counties with the best-fitting straight lines,* 1881-1900. Persons.

* See footnote to Diagram I.

for the first census one-tenth of the difference between that factor and the next in series multiplied by the difference in years between the first and the second census. For instance, the correction factor used for 1884 was the 1881 factor plus three-tenths of the difference between the 1881 and 1891 factors. The rates thus corrected were again expressed as index numbers and straight lines fitted (Diagram IIb and Table Ib).

The actual gradients are of course different in the two cases, but the relative positions of the counties are not so much affected as might have been surmised; nevertheless, as there are certain alterations, we shall confine our remarks to the age-corrected figures. At first sight it would appear that the gradients justify Dr. Stevenson's suggestion: we should anticipate that the London gradient would be the least, and so in fact

TABLE Ib.—CORRECTED DEATH-RATE FROM CANCER PER 1,000,000 LIVING FOR THREE URBAN AND TWO RURAL REGISTRATION COUNTIES,* 1881-1900. PERSONS.

Year	Urban Counties						Rural Counties			
	London		Lancashire		Northumberland		Lincolnshire		Somersetshire	
	Death-rates	Index Nos.†	Death-rates	Index Nos.†	Death-rates	Index Nos.†	Death-rates	Index Nos.†	Death-rates	Index Nos.†
1881	689	82	507	77	624	86	551	86	479	77
1882	710	85	505	76	571	79	531	83	504	81
1883	731	87	516	78	550	76	503	79	554	89
1884	729	87	527	80	519	72	510	80	546	87
1885	716	86	538	81	564	78	517	81	488	78
1886	723	87	558	84	643	89	518	81	582	93
1887	768	92	590	89	657	91	584	92	565	90
1888	758	91	622	94	698	97	556	87	623	100
1889	771	92	634	96	708	98	631	99	623	100
1890	868	104	671	101	739	102	649	102	638	102
1891	873	104	689	104	760	105	677	106	636	102
1892	832	100	698	105	779	108	627	98	637	102
1893	878	105	705	106	803	111	720	113	640	102
1894	880	105	707	107	770	107	685	107	651	104
1895	915	109	746	113	771	107	732	115	677	108
1896	936	112	762	115	824	114	755	118	674	108
1897	951	114	793	120	850	118	751	118	717	115
1898	978	117	820	124	848	117	698	109	712	114
1899	1001	120	817	123	846	117	782	123	786	126
1900	1007	120	843	127	914	127	776	122	765	122
Gradient of the best fitting straight line ‡	2·095		2·865		2·565		2·449		2·209	

* The crude death-rates for Wiltshire were so irregular, owing to the small size of the population upon which they were based, that corrected rates were not calculated for this County.

† *See* footnote (*) to Table Ia.

‡ *See* footnote (†) to Table Ia.

it is; but further examination reveals difficulties. The two rural counties have the next smallest gradients to London, Somerset not being very different from the metropolis, while the highly urbanized county of Lancashire has the steepest gradient in the series. It is difficult to suppose that Somerset and Lincoln represent a later phase

in the developing accuracy of diagnosis and certification than do Lancashire and Northumberland, and we must conclude that, if the principle of the method be accepted, the results are unfavourable to the simple hypothesis.

TABLE IIa.—CRUDE DEATH-RATE FROM CANCER PER 1,000,000 LIVING FOR THREE URBAN AND TWO* RURAL REGISTRATION COUNTIES, 1901-1910, MALES AND FEMALES.

Year	Urban Counties						Rural Counties			
	London		Lancashire		Northumberland		Lincolnshire		Somersetshire	
	Death-rates†	Index Nos.‡	Death-rates	Index Nos.‡	Death-rates	Index Nos.‡	Death-rates	Index Nos.‡	Death-rates	Index Nos.‡
Males—										
1901	798	82	593	88	734	97	762	88	785	92
1902	905	93	605	90	689	91	700	81	720	85
1903	936	96	629	94	690	92	815	95	834	98
1904	931	95	618	92	765	102	889	103	788	93
1905	930	95	688	102	717	95	927	108	861	101
1906	1028	105	687	102	741	98	918	107	740	87
1907	1022	105	666	99	721	96	932	108	874	103
1908	1055	108	713	106	777	103	896	104	1055	124
1909	1059	109	716	107	816	108	956	111	996	117
1910	1096	112	801	119	883	117	817	95	859	101
Gradient of the best fitting straight line§	2·897		2·903		2·055		2·012		2·733	
Females—										
1901	1066	94	905	98	964	104	984	86	1105	99
1902	1109	97	893	97	922	99	1061	93	1082	97
1903	1133	100	921	100	933	100	1140	100	991	89
1904	1122	99	879	95	872	94	1270	111	1170	105
1905	1128	99	892	97	938	100	1017	89	1063	95
1906	1170	103	941	102	942	101	1138	99	976	87
1907	1133	100	944	102	834	90	1168	102	1172	105
1908	1165	102	914	99	1008	108	1237	108	1137	102
1909	1196	105	975	106	940	101	1213	106	1189	106
1910	1158	102	976	106	961	103	1219	106	1305	117
Gradient of the best fitting straight line§	0·879		0·945		0·206		1·661		1·709	

* The County of Wiltshire has been omitted. When the two sexes were separated the yearly death-rates became very irregular owing to their being based upon a relatively small number of deaths.

† These death-rates differ slightly from those given in the Registrar-General's Annual Reports, as they have been recalculated, using more correct populations for the different years. The original figures were calculated assuming that the population of London between 1901 and 1911 had increased at the same rate as during the previous decennium, whereas, as a matter of fact, there had been an actual decrease of about 15,000 persons.

‡ *See* footnote (*) to Table Ia.

§ *See* footnote (†) to Table Ia.

Passing to the years 1901-10, for which the separate returns for males and females are available, Tables IIA and IIB contain the statistical results prepared in the same way as before. Again confining our attention to the age-corrected figures, we undoubtedly find a somewhat different state of affairs, since now there is a general distinction

TABLE IIB.—CORRECTED DEATH-RATE FROM CANCER PER 1,000,000 LIVING FOR THREE URBAN AND TWO RURAL REGISTRATION COUNTIES, 1901-1910. MALES AND FEMALES.

Year	Urban Counties						Rural Counties			
	London		Lancashire		Northumberland		Lincoln		Somerset	
	Death-rates*	Index Nos.†	Corrected death-rates	Index Nos.†	Corrected death-rates	Index Nos.†	Corrected death-rates	Index Nos.†	Corrected death-rates	Index Nos.†
Males—										
1901	864	87	699	93	806	101	602	89	615	95
1902	968	97	706	94	752	94	552	82	652	87
1903	989	99	725	97	747	93	642	95	647	100
1904	971	98	704	94	822	103	700	103	608	94
1905	958	96	774	103	764	96	729	108	660	102
1906	1045	105	763	102	783	98	721	107	564	87
1907	1026	103	731	98	756	95	731	108	663	102
1908	1046	105	773	103	809	101	702	104	796	122
1909	1036	104	767	102	843	106	749	110	748	114
1910	1058	106	847	113	905	113	639	94	641	98
Gradient of the best fitting straight line ‡	+1·661		+1·679		+1·273		+1·818		+2·030	
Females—	*									
1901	1092	98	1027	103	1098	106	838	87	875	102
1902	1125	101	1003	101	1044	101	901	94	850	99
1903	1139	102	1023	103	1050	102	964	101	773	90
1904	1117	100	965	97	975	94	1070	112	905	106
1905	1112	100	969	97	1043	101	854	89	816	95
1906	1143	102	1010	101	1041	101	952	99	744	87
1907	1096	98	1002	101	916	89	974	102	886	104
1908	1116	100	959	96	1100	107	1028	107	853	100
1909	1134	102	1011	101	1019	99	1004	105	884	103
1910	1087	97	1000	100	1036	100	1006	105	963	113
Gradient of the best fitting straight line ‡	−0 097§		−0·279§		−0·352§		+1·576		+0·921	

* These rates have been obtained by correcting the rates for London given in Table IIA, and not the rates given by the Registrar General in his Annual Reports.

† *See* footnote (*) to Table IA.

‡ *See* footnote (†) to Table IA.

§ The negative sign indicates that the death-rate has been decreasing.

between the urban counties on the one hand and the rural on the other. For males, all the urban lines show a less steep gradient than do the rural ones, precisely as we should anticipate on the simple hypothesis, and there is a similar distinction in the case of females. Were these the only results before us, we should conclude that Dr. Stevenson's

TABLE III.—CORRECTED DEATH-RATE * PER 1,000,000 PERSONS LIVING OVER 35 YEARS OF AGE FROM CANCER IN AGGREGATES OF URBAN AND RURAL COUNTIES, 1901-1910. MALES AND FEMALES.

Year	MALES				FEMALES			
	Urban Counties		Rural Counties		Urban Counties		Rural Counties	
	Death-rates	Index Nos.†	Death-rates	Index Nos.†	Death-rates	Index Nos.†	Death-rates	Index Nos.†
1901	2218	89	2006	91	2993	96	2681	95
1902	2254	90	1957	89	3030	97	2757	97
1903	2357	95	2064	94	3101	99	2685	95
1904	2371	95	2092	95	3033	97	2831	100
1905	2434	98	2189	100	3068	98	2739	97
1906	2592	104	2193	100	3166	101	2789	98
1907	2518	101	2244	102	3143	101	2897	102
1908	2644	106	2303	105	3143	101	2856	101
1909	2656	107	2665	121	3258	104	3105	109
1910	2865	115	2285	104	3273	105	3022	107
‡ Gradient of the best fitting straight line	2·596		2·513		0·923		1·412	

* These rates were taken from the Registrar-General's Annual Reports. Up to 1908 the death-rates per 1,000,000 of persons over 35 years of age were given; but after that date for all ages. In the Report for 1908, however, the average corrected rate for *all ages* for the period 1903-07 is given, and can be compared with the corrected rate for the same period for *ages over 35 only*. Hence the value of the ratio

$$\frac{\text{Death-rate over 35 from cancer, 1903-07}}{\text{Death-rate at all ages from cancer, 1903-07}}$$

was obtained, and the death-rates for years after 1907 were multiplied by this factor to make them comparable with those of the earlier years. The actual values of this ratio were: Males, Urban, 3·064, Rural, 3·067; Females, Urban, 2·946, Rural, 2·960.

† *See* footnote (*) to Table Ia.

‡ *See* footnote (†) to Table IIa.

hypothesis ought to be accepted. In view, however, of the divergent indications afforded by the previous series, based as they were upon a longer experience, we do not feel justified in assenting to this conclusion. Even here there are certain peculiarities; London for instance no longer has the least gradient, its position in the old series is occupied

now by Northumberland, so that if we are to trust the later results we must confine ourselves to contrasting urban with rural counties, in which event the only circumstance favourable to the hypothesis in the 1881-1901 series—viz., the position of London—loses any importance it may have possessed.

Assuming that the comparison should be made between all urban and all rural districts, we ought to be able to decide between our two series by an aggregate comparison. The corrected rates for ten years

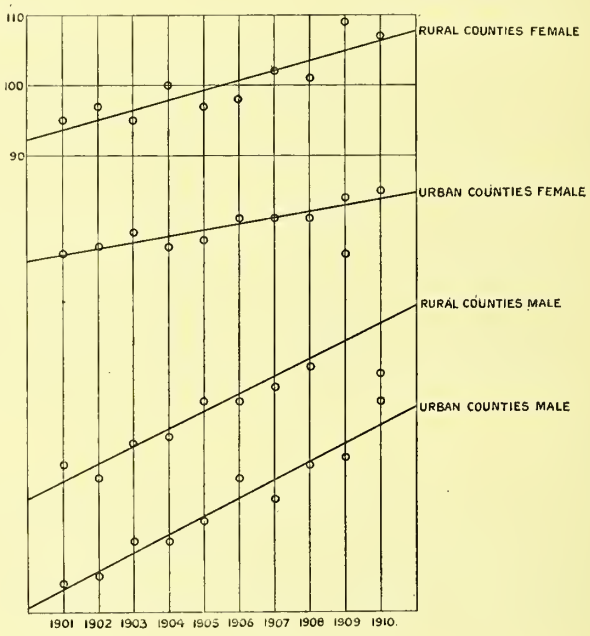


Diagram III.

Index numbers showing the changes in the death-rates from cancer for urban and rural counties, with the best fitting straight lines, 1901-10. Males and females.

appear with the appropriate constants in Table III. It will be seen that the gradients of the lines for males do not differ essentially; the line for rural females has, however, ascended more steeply than that for the females in urban districts. It may perhaps be said that the results of this comparison rather confirm the 1881-1901 series, but the course of the rates is perplexing. Thus the males in rural districts show a marked decline in 1910, as compared with 1909, a phenomenon not seen in the urban districts nor among the females.

The results of another tentative inquiry are of interest here. Assuming that the proportion of deaths occurring in institutions where autopsies may be performed is some criterion of diagnostic facilities, we thought it would be well to compare changes in the proportions. The Registrar-General's Annual Reports for 1881, 1886, 1891, 1896, 1901 and 1906 were searched and the deaths occurring in hospitals, nursing

TABLE IV.—TABLE SHOWING THE NUMBER OF ADULTS DYING IN GENERAL HOSPITALS IN SIX SELECTED REGISTRATION COUNTIES,* 1881-1906.

County	Number of hospitals	Number of adults dying in general hospitals	Total number of deaths of persons aged 1 year and upwards	Percentage of hospital deaths to all deaths over 1 year	County	Number of hospitals	Number of adults dying in general hospitals	Total number of deaths of persons aged 1 year and upwards	Percentage of hospital deaths to all deaths over 1 year
London—					*Lincolnshire*—				
1881	50	4,991	61,698	8·1	1881	7	84	5,994	1·4
1886	60	5,528	61,227	9·0	1886	9	92	6,348	1·4
1891	72	6,529	68,346	9·6	1891	7	95	7,052	1·3
1896	66	6,671	59,225	11·3	1896	7	81	5,574	1·5
1901	72	7,389	58,546	12·6	1901	9	147	5,792	2·5
1906	75	8,358	55,746	15·0	1906	10	229	5,920	3·9
Lancashire—					*Somersetshire*—				
1881	30	1,360	57,037	2·4	1881	12	217	6,834	3·2
1886	37	1,787	61,855	2·9	1886	16	244	7,214	3·4
1891	44	2,463	71,078	3·5	1891	17	250	7,768	3·2
1896	47	2,396	61,469	3·9	1896	15	240	6,345	3·8
1901	50	2,912	63,520	4·6	1901	16	228	5,767	4·0
1906	57	3,465	62,492	5·5	1906	16	273	5,563	4·9
Northumberland—					*Wiltshire*—				
1881	4	147	6,195	2·4	1881	5	31	3,531	0·9
1886	8	227	6,748	3·4	1886	8	52	3,564	1·5
1891	7	280	8,125	3·4	1891	8	64	3,609	1·8
1896	8	345	6,893	5·0	1896	9	87	2,963	2·9
1901	10	490	8,280	5·9	1901	11	70	3,129	2·2
1906	14	539	8,293	6·5	1906	12	134	3,228	4·2

* Deaths occurring in Workhouses, Workhouse Infirmaries, Isolation Hospitals, Lying-in Hospitals, Children's Hospitals and Hospitals for Special Diseases other than cancer have not been included.

or surgical homes and infirmaries (other than workhouse infirmaries) were extracted. We excluded Poor Law infirmaries, children's hospitals and hospitals for infectious diseases. The proportion of deaths in the selected institutions to all deaths at ages over 1 was computed in the case of each county and the results appear in Table IV.

It will be seen that the proportion is far higher in London than elsewhere, but the relative increase is greater in the other districts excepting Somerset. Were we to go by this test we should certainly

infer that the means of diagnosis began on a higher level in London but improved less rapidly there than elsewhere; hence, on our original hypothesis, we should expect the London cancer gradient to be less steep than in the other districts excepting Somerset. As we have seen, this is not the case. But we are not disposed to attach very much importance to this test. One objection is that we may have failed to select the right institutions; another is that the number of institutional deaths is absolutely so small, except in the metropolis, that it can have had very little direct influence upon the local cancer death-rate. It is, of course, possible that the existence of quite a small institution providing a centre at which operations may be performed and some pathological work carried out, makes more difference to the general level of knowledge in its vicinity than could be inferred merely from the number of patients admitted to and dying within its walls, but we have no statistical evidence that this is so.

On the whole, as we have said, we seem to be justified in answering "no" to Dr. Stevenson's question—"Whether England and Wales in 1911 do not compare with England and Wales in 1881 more or less as London in 1911 does with rural districts in 1911?"

The course of the changes seems to be inconsistent with so simple an hypothesis. It must be clearly understood, as stated on p. 11 *supra*, that our method cannot disprove the general statement that the recorded increase in cancer rate is a result of improved diagnosis, since we do not know that the improvements have not followed a different law in the different types of district; but a presumption against this is created by the failure of the great urban counties uniformly to differentiate themselves from the rural counties in virtue of their rates of change. This, however, is an argument we cannot push far, for in the 1901-11 experience there is some differentiation, and the most we claim is to have made out a case against the simplest and most intelligible form of the explanation by diagnosis which has been put forward.

We now turn to a group of inquiries directed more particularly to the question of diagnosis. We first considered the ratio of deaths from old age to deaths from cancer. Assuming that the real incidence of cancer is constant and that where facilities for making a diagnosis are poor many deaths from cancer would be certified as due to old age, it seemed that a knowledge of the changes in this ratio would be of interest. Straight lines were fitted as before to the London, Somerset, Lincoln and Lancashire ratios for 1890-1910. There are no very sharp

differences, nor do such as appear fall into a regular order. London has indeed the steepest gradient, but it is closely followed by Somerset, which has a steeper line than the highly urbanized county of Lancashire (Table V).

TABLE V.—CHANGE IN THE RATIO OF DEATHS FROM OLD AGE TO DEATHS FROM CANCER FOR FOUR REGISTRATION COUNTIES. MALES. 1890-1910.

Year	Urban Counties				Rural Counties			
	London		Lancashire		Lincolnshire		Somersetshire	
	Deaths from old age / Deaths from cancer	Index Nos.*	Deaths from old age / Deaths from cancer	Index Nos.*	Deaths from old age / Deaths from cancer	Index Nos.*	Deaths from old age / Deaths from cancer	Index Nos.*
1890	0·777	139	1·650	144	2·197	130	2·248	135
1891	0·802	143	1·470	128	2·083	125	2·217	134
1892	0·730	130	1·441	126	2·141	127	2·396	144
1893	0·762	136	1·260	110	1·632	97	1·736	105
1894	0·535	95	1·109	97	1·671	99	1·732	104
1895	0·640	114	1·321	115	1·896	112	2·103	127
1896	0·527	94	1·200	105	1·518	90	1·474	89
1897	0·518	92	1·103	96	2·006	119	1·481	89
1898	0·530	94	1·042	91	1·775	105	1·650	99
1899	0·595	106	1·245	109	1·869	111	1·762	106
1900	0·577	103	1·197	105	1·746	104	1·695	102
1901	0·590	105	1·056	92	1·714	102	1·659	100
1902	0·496	88	1·070	93	1·708	101	1·641	99
1903	0·441	79	1·012	88	1·485	88	1·271	77
1904	0·504	90	1·097	96	1·486	88	1·630	98
1905	0·487	87	0·983	86	1·357	80	1·383	83
1906	0·469	84	1·039	91	1·607	95	1·765	106
1907	0·458	82	1·024	89	1·368	81	1·370	83
1908	0·416	74	0·953	83	1·383	82	1·116	67
1909	0·497	89	0·958	84	1·342	80	1·368	82
1910	0·433	77	0·813	71	1·436	85	1·143	69
† Gradient of the best fitting straight line	−2·858 ‡		−2·432 ‡		−2·116 ‡		−2·743 ‡	

* *See* footnote (*) to Table Ia.

† *See* footnote (†) to Table IIa.

‡ The negative sign indicates that there has been a decrease in the value of the ratio.

None of the lines, however, fit the observations at all closely, which is not surprising, since weather conditions must influence the true death-rate among aged persons and such effects may well be sufficient to mask any regular transfer of certain deaths formerly attributed to senility to another group. We think the results of this particular test must be regarded as purely negative.

We now turn to the rates for different sites and begin with the experience of England and Wales, 1897-1911. The data used were read off from the diagrams on pp. lxix and lxx of the Registrar-General's Annual Report for 1910. The sites selected for analysis were, in males, intestine, tongue, rectum, stomach, liver; in females, intestine, rectum, breast, stomach, uterus and liver. Lines have been fitted to index numbers derived both from corrected and uncorrected rates, except in the instance of the liver, which we only included as a possible criterion of changes in diagnosis. Tables VIA and VIB and Diagrams IVA, IVB, VA and VB were constructed on the same principles as before. Beginning with the males (corrected figures) we notice that the stomach-rate has increased less than that of the tongue, which is represented by a line of sensibly the same gradient as the rectum; the rate of intestinal cancer has advanced faster than any of the others. Among females, we find a similar rate of increase both for the breast and the stomach, a slightly greater rate of change in the rectum and a markedly steeper gradient for the line of intestinal cancer. The slope of the uterine rates is actually negative. The position of intestinal cancer can easily be explained on the hypothesis of improved diagnosis, but the other results seem to us quite inconsistent with this supposition. Cancer of the tongue in males and of the breast in females, typically accessible sites, have increased faster than cancer of the stomach, the difference in the case of the tongue being quite appreciable. Cancer of the rectum, again, has not advanced very much, if at all, faster than that of the accessible sites. It is a point not without significance that cancer of the rectum has in both sexes increased faster than cancer of the stomach, although the former has always been presumably easier to diagnose than the latter.

The decrease in uterine cancer cannot be passed over in silence. Bashford and Murray's figures seem to suggest that diagnosis of uterine cancer is subject to as much alteration on the findings of an autopsy as malignant disease of other sites, so that with an assumed constant incidence and improved diagnosis we ought to have had an increase in cancer of this site too. Are we to suppose either (1) that diagnosis in the particular instance of cancer of the uterus has deteriorated or (2) that owing to operative improvements the true death-rate from the disease has diminished, the incidence remaining the same? We can of course discard (1), but (2) is impossible to answer. There is no doubt that more persons, at least in London, now resort to hospitals than before, and it is generally believed that both the technique and

TABLE VIa.—CRUDE DEATH-RATE PER 1,000,000 LIVING FROM CANCER OF CERTAIN SPECIFIED ORGANS OF THE BODY, 1897-1911. MALES AND FEMALES.

(*A*) *Males.*

Year	Specified Organs									
	Stomach		Intestine		Rectum		Liver		Tongue	
	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*
1897	131	82	40	67	55	74	90	95	32	77
1898	139	87	40	67	62	84	87	92	33	80
1899	137	86	45	76	61	82	89	94	36	87
1900	138	87	47	79	61	82	95	101	37	90
1901	150	94	50	84	66	89	95	101	36	87
1902	150	94	50	84	72	97	95	101	40	97
1903	160	101	51	86	78	105	99	105	40	97
1904	165	104	55	93	77	104	96	102	38	92
1905	165	104	61	103	75	101	95	101	45	109
1906	171	107	69	116	81	109	97	103	43	104
1907	164	103	64	108	85	115	91	96	44	106
1908	169	106	70	118	85	115	95	101	47	114
1909	175	110	80	135	80	108	98	104	49	119
1910	183	115	82	138	86	116	97	103	49	119
1911	190	119	85	143	88	119	98	104	51	123
Gradient of the best fitting straight line	2·426		5·568		3·074		0·563		3·188	

(*B*) *Females.*

Year	Specified Organs											
	Stomach		Intestine		Rectum		Liver		Uterus		Breast	
	Death-rates	Index Nos.	Death-rates	Index Nos.	Death-rates	Index Nos.	Death-rates	Index Nos.	Death-rates	Index Nos.	Death-rates	Index Nos.
1897	124	87	49	65	50	85	124	92	217	98	143	86
1898	125	88	49	65	50	85	128	94	220	100	148	89
1899	130	92	59	79	50	85	140	103	215	97	152	91
1900	135	95	56	75	52	88	140	103	220	100	154	92
1901	134	94	68	91	55	93	140	103	230	104	155	93
1902	139	98	63	84	56	95	134	99	231	105	165	99
1903	145	102	68	91	62	105	134	99	228	103	171	102
1904	142	100	71	95	60	102	136	100	226	102	172	103
1905	140	99	74	99	64	108	135	100	227	103	168	101
1906	152	107	85	114	63	107	137	101	222	100	169	101
1907	147	103	85	114	62	105	138	102	220	100	175	105
1908	150	106	88	118	65	110	133	98	215	97	177	106
1909	157	111	96	128	67	113	136	100	222	100	185	111
1910	155	109	96	128	65	110	137	101	219	99	185	111
1911	156	110	115	154	65	110	140	103	203	92	184	110
†Gradient of the best fitting straight line	1·657		5·357		2·231		0·314		− 0·223‡		1·779	

* *See* footnote (*) to Table Ia.
† *See* footnote (†) to Table IIa.
‡ The negative sign indicates that the rate has decreased.

TABLE VIb.—CORRECTED DEATH-RATE PER 1,000,000 LIVING FROM CANCER OF CERTAIN SPECIFIED ORGANS OF THE BODY, 1897-1911. MALES AND FEMALES.

(*A*) *Males.*

Year	Specified Organs							
	Stomach		Intestine		Rectum		Tongue	
	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*
1897	132	87	40	71	55	78	32	82
1898	140	92	40	71	62	88	33	85
1899	138	90	45	80	61	87	36	92
1900	138	91	47	84	61	86	37	95
1901	150	99	50	89	66	93	36	92
1902	148	98	49	88	71	101	40	101
1903	156	103	50	89	76	108	39	100
1904	159	105	53	94	74	105	37	93
1905	158	104	58	104	72	101	43	109
1906	161	106	65	116	76	108	40	103
1907	153	100	60	106	79	112	41	104
1908	156	102	65	115	78	111	43	109
1909	159	105	73	129	73	103	44	112
1910	164	108	74	131	77	109	43	111
1911	168	111	76	134	78	110	44	113
Gradient of the best fitting straight line †	1·439		4·455		2·121		2·064	

(*B*) *Females.*

Year	Specified Organs									
	Stomach		Intestine		Rectum		Uterus		Breast	
	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*	Death-rates	Index Nos.*
1897	125	90	49	68	51	87	219	101	144	88
1898	126	90	49	68	50	87	222	102	149	91
1899	130	94	59	81	50	87	216	99	153	93
1900	135	97	56	77	52	90	221	101	154	94
1901	134	96	68	93	55	95	230	106	155	95
1902	138	99	63	86	56	96	230	106	164	100
1903	143	103	67	92	61	106	226	104	169	103
1904	140	100	70	96	59	102	223	102	169	103
1905	137	98	72	99	63	108	223	102	164	100
1906	148	106	83	113	61	106	217	100	164	101
1907	142	102	82	113	60	104	214	98	169	104
1908	144	104	85	116	63	108	208	96	170	104
1909	150	108	92	126	64	110	214	98	177	108
1910	147	106	91	125	62	107	210	96	176	108
1911	147	106	109	149	62	107	194	89	174	106
Gradient of the best fitting straight line†	1·189		5·039		1·771		−0·654 ‡		1·318	

* *See* footnote (*) to Table Ia.

† *See* footnote (†) to Table Ia.

‡ The negative sign indicates that the rate has diminished.

after-results of operations have been greatly improved; but it is also true that a large, undetermined, proportion of hospital patients are first seen when the disease has advanced so far as to be inoperable. We have, therefore, to set against the saving of lives due to operation the addition to the death-rate of inoperable cases, some of which might not have been diagnosed apart from the hospital. Whether the accounts

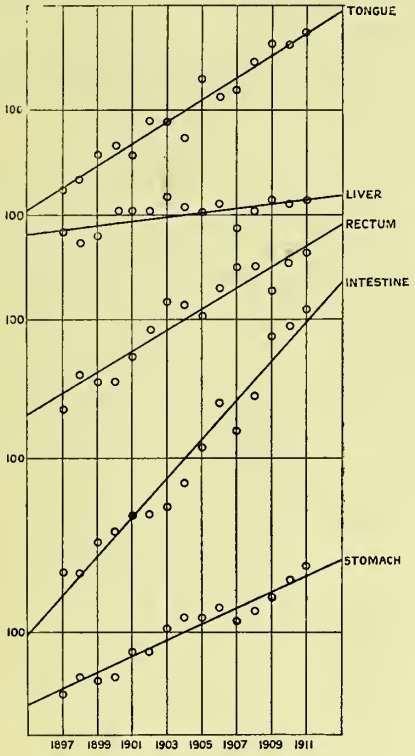


Diagram IVa.

Index numbers showing the changes in the crude death-rates from cancer of certain organs of the body with the best fitting straight lines, 1897-1911. Males.

balance is a point we have no means of deciding. Even so apparently simple a matter as the saving of lives by operation is difficult to study, because all surgeons do not adopt the same criterion of "cure" (*see* Lewers, pp. 141-142).

The general conclusion deducible from this analysis of site-rates

seems to us to be that such changes as have occurred cannot all be attributed to improved diagnosis combined with a steady incidence of the disease, and we might quote here a remark from the Registrar-General's Report for 1909 (p. xciii): "The increase amongst males from cancer of the jaw, and especially of the tongue, is remarkable, and can scarcely be explained by improved diagnosis. Although cancer of the tongue in its later stages presents little difficulty in diagnosis, the recorded mortality has increased amongst males by no less than

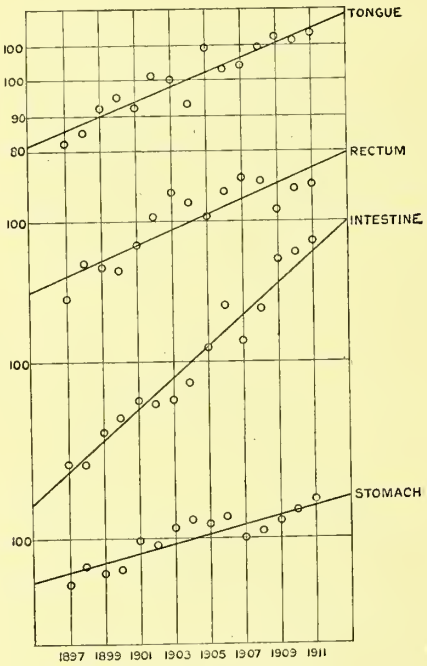


DIAGRAM IVB.

Index numbers showing the changes in the corrected death-rates from cancer of certain organs of the body with the best fitting straight lines, 1897-1911. Males.

228 per cent. in forty-one years. Moreover, the increase is entirely confined to the male sex." Because there has been a real increase in the frequency of cancer of one organ it does not follow that there has been a general increase of the cancer rate; but such an admission should make one cautious in attributing changes in the rate for another organ to a cause group which cannot be directly measured, and does not appear to be operative in the former case.

We next approached the problem from a different side. Let us suppose that the rate of cancer ascribed to the liver in any district is a measure of the diagnostic facilities of that district, that where the liver-rate is high the means of establishing a diagnosis are poor; do we find that where this rate is high the rate for all other sites is low? The data we used were derived from twenty-two Swiss cantons (the

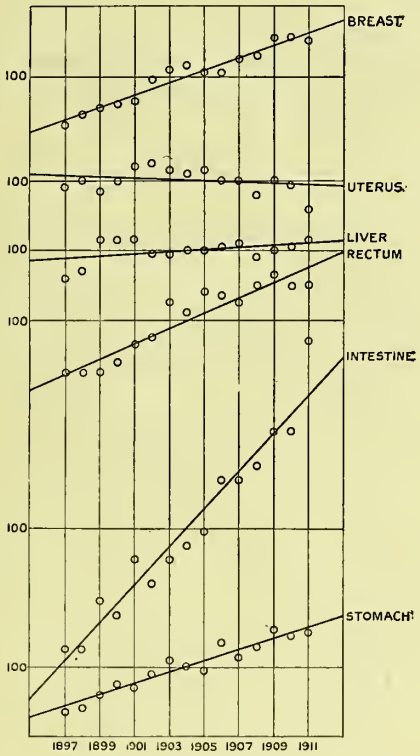


Diagram Va.

Index numbers showing the changes in the crude death-rates from cancer of certain organs of the body with the best fitting straight lines, 1897-1911. Females.

two Appenzell cantons and the two Unterwalden cantons were grouped together, and Uri was omitted owing to its small size), the rates being calculated from five years' returns (females); we could not separate cancer of the liver from cancer of the gall-bladder, which was unfortunate, since the latter condition would not in most cases be a mere secondary deposit, but it is certain that the bulk of the cases must have been cancer of the

liver. The rates were corrected for age-distribution, but our standard rate of mortality refers to all forms of cancer, so that the site-rates could not be separately corrected. Working out the correlation between the liver rate and that for all sites other than the liver, we reach $r = 0{\cdot}276 \pm 0{\cdot}133$, a result inconsistent with any really appreciable negative correlation between the variables. In other words, if the liver-rate be a measure of poor diagnosis, the latter is not negatively

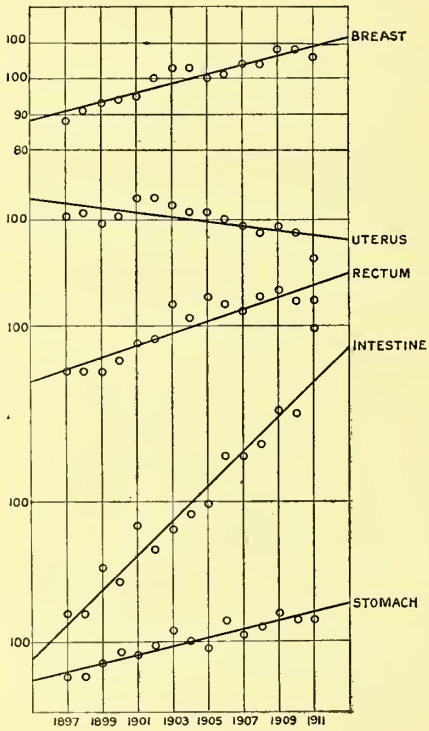


Diagram Vb.

Index numbers showing the changes in the corrected death-rates from cancer of certain organs of the body with the best fitting straight lines. 1897-1911. Females.

associated with the rate of mortality from all other forms of cancer (Table VII). We next measured the correlation between (*a*) the rates of two inaccessible groups, stomach with rectum and intestine, and between (*b*), an inaccessible and an accessible group, stomach with uterus and breast. The values are $0{\cdot}263 \pm 0{\cdot}134$ and $0{\cdot}127 \pm 0{\cdot}142$; we cannot say that the difference, in view of the probable errors, is

of importance. We have also correlated the liver-rate with that of the stomach, uterus and breast, rectum and intestine, surmising that were the liver-rate a measure of diagnosis the values would be less negative in the case of accessible than of inaccessible sites. The values are: 0·161 ± 0·140 (liver with stomach), 0·145 ± 0·141 (liver with rectum and intestine), and 0·099 ± 0·142 (liver with uterus and breast). The differences are quite without significance.

TABLE VII.—CORRELATION BETWEEN THE CORRECTED DEATH-RATES FROM CANCER OF DIFFERENT SITES OF THE BODY FOR TWENTY-TWO SWISS CANTONS. FEMALES.

Variables	Correlations
Death-rate from cancer of the liver and cancer of all sites other than the liver	+0·2761 ± 0·1328
Death-rate from cancer of the liver and cancer of the stomach ...	+0·1608 ± 0·1401
Death-rate from cancer of the liver and cancer of the rectum and intestine	+0·1454 ± 0·1408
Death-rate from cancer of the liver and cancer of the uterus and breast	+0·0994 ± 0·1424
Death-rate from cancer of the stomach and cancer of the rectum and intestine	+0·2631 ± 0·1338
Death-rate from cancer of the stomach and cancer of the uterus and breast	+0·1268 ± 0·1415

Means and Standard Deviations.

Variable	Mean rate *	Standard deviation	Variable	Mean rate *	Standard deviation
Corrected death-rate from cancer of the—			Corrected death-rate from cancer of the—		
Liver	12·292	4·582	Rectum and intestine	10·937	3·967
All sites except liver	110·364	23·915	Uterus and breast ...	32·255	8·740
Stomach... ...	47·518	19·527			

* Rate per 100,000 based upon the mean of five ycars.

Our next investigation is merely recorded as a suggestion, since it depends upon a hypothesis the truth of which is very doubtful. Assuming that diagnosis is approximately uniform throughout Switzerland, then if variations in the general rate of cancer mortality in that country are mainly or largely dependent upon variations in the rates for inaccessible sites, it is plain that the onus of proof is upon those who assert that similar variations in other countries are the effects of differences in the accuracy of certification or diagnosis.

We find in Switzerland that the variability of the stomach-rate is considerably higher than that of uterus and breast combined. The correlation is 0·127, the percentage regression of stomach upon uterus and breast is 0·192 and of uterus and breast upon stomach 0·084. If the stomach-rate be increased 1 per cent. of its mean value, the predicted change of breast- and uterus-rate will be 0·084 per cent. of their mean. Conversely if the uterus-breast rate be increased 1 per cent., the predicted change of the stomach rate will be 0·192 per cent. of its mean. On the above hypothesis these results show, we think, that there is little interdependence between changes in the two rates, and afford an additional argument in support of the view that circumstances, apart from diagnosis, which affect one rate will not influence the other. With regard to the proposition that variations in the general rate chiefly depend upon variations in certain sites, we may refer to the paper of Prinzing cited above.[1] We may add that although we have here discussed the problem which is of most popular interest—viz., changes in the *general* rate of cancer mortality—we conceive that a more fruitful line of research is to analyse the local and secular changes in the rates of particular sites, in view of their largely independent variability.

This concludes our series of tests, and we may proceed to sum up the case presented. The argument that the recorded increase of cancer is not a consequence of a really increased incidence is founded upon three main propositions, viz.—(1) The manner in which the male rate has overtaken the female rate; (2) the marked increases in the rates for certain inaccessible sites; (3) the exceptional position of London in respect of cancer mortality among males.

With respect to (1) we think we have shown that this change in relative position of the sexes is difficult to reconcile with so simple an explanation as that diagnosis is easier in the case of females and earlier became constant; there is reason to doubt whether the rate for even so accessible a site as the uterus is not sensibly affected by facilities for precising a diagnosis, and further the very different courses pursued by the uterus and breast rates require to be explained.

With respect to (2) we have shown that the rate of increase is not uniformly greater in the case of inaccessible sites, that in particular the rates for the tongue in males, and for the breast in females, have advanced in a manner inconsistent with the *general* validity of the proposition as applied to all forms of cancer, with a consequence that

[1] *See* also Kolb's paper.

the attribution of the great increase in certain other inaccessible site rates to improvements in diagnosis becomes arbitrary.

With respect to (3) our study of the county rates seems to prove that the position of London is not exceptional, and that in particular we can hardly suppose that extra-metropolitan districts are now upon the plane occupied by London twenty or thirty years ago.

TABLE VIII.—CORRECTED DEATH-RATE PER 100,000 LIVING FROM CANCER OF DIFFERENT SITES OF THE BODY IN TWENTY-TWO SWISS CANTONS BASED UPON THE MEANS OF THE FIVE YEARS, 1901-05. FEMALES.

Canton	Corrected Death-rate from Cancer of the following Sites				
	Liver	All sites except liver	Stomach	Rectum and intestine	Uterus and breast
Zürich	12·74	144·8	67·8	12·18	40·8
Bern	10·62	93·8	30·8	9·54	31·6
Luzern	24·06	118·2	52·8	10·46	30·2
Schwyz	17·00	111·0	49·0	11·10	22·8
Obwalden } Niedwalden }	8·60	115·6	57·8	7·38	21·0
Glarus	18·20	101·8	25·6	9·64	30·0
Zug	13·44	107·2	97·2	11·96	34·4
Freiburg	8·36	100·6	38·2	9·24	34·6
Solothurn	11·22	111·4	46·4	12·82	31·6
Basel Stadt	13·54	128·2	42·8	15·36	40·6
Basel Land	16·72	83·6	21·0	5·58	34·6
Schaffhausen	17·56	109·6	45·2	13·38	34·2
Appenzell a/R } Appenzell i/R } ...	11·36	163·6	91·6	14·52	36·6
St. Gallen	12·56	127·0	66·6	8·66	26·6
Graubünden	7·78	99·8	44·0	8·10	26·2
Aargau	15·22	97·6	45·8	9·08	26·0
Thurgau	14·00	118·6	56·6	10·04	30·4
Tessin	4·80	78·4	33·8	5·66	25·4
Waadt	8·98	112·8	40·4	13·18	38·2
Wallis	4·00	43·2	16·6	3·66	12·6
Neurenburg	10·84	128·6	36·8	20·60	47·4
Genf	8·82	132·6	38·6	18·48	53·8

Finally, the Swiss data, so far as they go, cause us to think that the scientific importance of comparisons between the rates for all forms of cancer is less than the general public, or even the medical profession as a whole suppose. This belief leads us to support the policy of the Registrar-General, advocated by the authorities of the Imperial Cancer Research Fund, of publishing separate tabulations of the site-rates, and to advocate the carrying of this tabulation even further than has yet been done—e.g., by the introduction of tables showing the site-rates in some of the larger administrative subdivisions. The general

conclusion to be drawn from our work is, we think, that there is some truth in the popular opinion that the real incidence of cancer has increased. We do not desire to minimize the importance of the sources of error pointed out by King, Newsholme, Bashford, Murray and Stevenson, and we are conscious that our statistical method is by no means so precise as we could wish. The general tenour of the results does, nevertheless, create a presumption in favour of the conclusion we have enunciated. We may, at least, claim that a sufficiently strong case has been established to render the alleged increase of cancer a suitable topic for discussion by a learned society rather than a mere illustration of the fallacies resulting from the unskilled treatment of medical statistics. If this claim be admitted, the purpose of our paper has been fulfilled.

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DISCUSSION.

The PRESIDENT (Dr. W. H. Hamer) said Mr. Greenwood had informed him that the paper would only contain "just what was to be expected"; but the Section would agree with him that, from the two authors concerned, the Section was entitled to expect the unexpected, and, in point of fact, they had enjoyed a treatment of the subject more original and unexpected than they could possibly have anticipated would be presented to them. He was, in any case, inclined to doubt the correctness of the view that the ordinary practitioner was not better able to diagnose cancer now than he was fifty years ago; but even if the statement were correct, there remained the very important consideration that what was in a practitioner's mind might, at one and another time, be varyingly expressed upon a death certificate. Changes of type and changes in recorded amounts of prevalence, as had been well said, depended upon the mental revolutions of practitioners far more than upon the actual revolutions of disease. Statistical methods could only be applied to the particulars recorded, and it was important they should recognize to how small an extent the records represented actual facts. A little London girl who visited a dairy farm in the country said she understood now how they got the milk out of the cow, but she would like to know how they got the milk into the cow. A similar sort of difficulty was apt to assume very great importance in connexion with most of the problems of medical statistics, and in this respect at least cancer was a disease presenting quite exceptional peculiarities. In the first place, in cancer more than almost any other disease the question arose as to whether the diagnosis should be revealed, and opinion in this matter had apparently undergone considerable change in the last forty or fifty years. Then, in the second place, cancer was a chronic disease, and patients were apt to die from other causes. In the somewhat similar case of phthisis it was interesting to find the fact stated in Dr. R. Dudfield's last annual report that on nearly a fifth of the death certificates of individuals known to him to have been reported to be suffering from phthisis, the primary, and in some the only, certified cause of death was a disease other than tuberculosis. And then in the third place, there was the question of operation. Fifty years ago operative interference was rare, nowadays quite a large percentage of cases underwent operation. The "suggestions" made to certifying practitioners by the Registrar-General on the back of the death certificate made special reference to the procedure to be adopted in filling up the certificate when death occurred after operation. This, in itself, must have led to considerable addition to the number of instances in which the fact that a patient had suffered from cancer found expression on the death certificate. For these reasons he (the President) thought that the likelihood of the diagnosis formed in the mind of the

practitioner being actually placed upon paper, and of cases of cancer thus being caught in the Registrar-General's net, was far greater now than it was fifty years ago; this was so even if it were assumed—and it was a very doubtful assumption—that the diagnostic skill of the average practitioner had not greatly improved. He would call upon Dr. Stevenson to open the discussion.

Dr. T. H. C. STEVENSON said he had listened with much interest to Mr. Greenwood's and Mrs. Wood's paper, on which he had only one or two remarks to offer, and those chiefly relating to his own personal connexion with a portion of its subject-matter. From the manner in which his contribution on the subject of cancer to the last Annual Report of the Registrar-General had been referred to in the paper, he was a little afraid that it might appear that he had there taken up a definite position in favour of the view that cancer mortality is not increasing in this country, and that he claimed to support this view by arguments deduced from the figures there quoted. So far from this being the case, he believed that the one fact with regard to this question which emerged with something approaching certainty from the study of the official records was that cancer of certain parts of the body—the tongue at all events, if not others—had increased, and in view of this fact very strong evidence would be required to support a definite conclusion against the increase of cancer in general. With the permission of the meeting he proposed to read some of the context in which his question, quoted by the authors of the paper, was set. "While the figures for a single year would be far too small a basis for forming a definite conclusion on this matter, and there is evidence pointing to a real increase of cancer of certain parts of the body, the figures for 1911 seem to harmonize sufficiently well with the hypothesis that recorded differences in mortality depend upon varying degrees of accuracy in diagnosis to make it worth while to watch those of subsequent years from the same point of view. If these latter point in the same direction then we must ask ourselves whether E. and W. in 1911 does not compare with E. and W. in 1881 more or less as London in 1911 does with the rural districts in 1911." It would be seen that the question was not even put, but merely suggested as one which, under certain circumstances, might have to be put in the future. To bring forward an elaborate array of statistical calculations in order to counter such a hypothetical possibility of a question as this, seemed to him like using a very heavy steam-hammer to crush a very small nut. He wished also to refer to the suggestion that the mortality records of different areas should be tabulated according to the part of the body affected. There would be no difficulty in doing so for the years 1911 onwards, when the system had come into operation in the General Register Office by which each death registered was represented by a card, which in the case of cancer bore a record of the part of the body affected. It would hardly be worth while, perhaps, to undertake such work until the records of a few years had accumulated, unless the areas compared

were to consist of large aggregations of districts, but in any case, the cards were at the disposal, at any time, of Mr. Greenwood or of any other responsible investigator who chose to take the matter up. To his mind, indeed, this feature of the 1911 Report of the Registrar-General was of far greater importance in its bearing on the cancer problem than any theorizing as to cancer increase, since it meant that the national records had for the first time been put into an adaptable form which would permit of valuable investigations such as that now suggested. In conclusion, he wished to associate himself with all that had been said by the President as to the interest and value of the paper.

Dr. E. F. BASHFORD : In the first place I should like to associate myself with the remarks of our Chairman and Dr. Stevenson on the value of Mr. Greenwood's and Mrs. Wood's paper. To those who, like myself, are deeply immersed in the study of cancer, the accession of valuable outside students is especially important, because of the very great caution we exhibit in expressing decided opinions. Mr. Greenwood concludes his paper by stating "a sufficiently strong case has been established to render the alleged increase of cancer a suitable topic for discussion by a learned society rather than a mere illustration of the fallacies resulting from the unskilled treatment of medical statistics." When King and Newsholme published their paper there was great necessity for exposing the fallacies resulting from the unskilled treatment of medical statistics, and the same necessity obtained when the investigations of the Imperial Cancer Research Fund were instituted. I agree with Mr. Greenwood that the publication in 1893 of a paper by King and Newsholme marked an epoch in the discussion of the question of the increase of cancer. Previously the discussion had been carried on somewhat loosely and largely by surgeons who certainly had not the accurate knowledge of the pathology of the disease which we possess to-day, and often were ignorant of statistical methods. In view of the more extensive data available to-day, the value of King and Newsholme's paper appears to me to lie, not in discussing whether they were right or wrong in concluding in 1893 that the increase of cancer was apparent and not real, but in the stimulus they gave to accurate discussion. They showed that the matter could only be discussed by comparing the deaths from cancer according to the numbers living in each age-period for the two sexes separately in the several decennia compared. They insisted that the difficulties in diagnosing and certifying cancer as a cause of death were greater for some parts of the body than for others, by grouping them into what they termed " accessible and inaccessible " sites. Of course the incidence of cancer had been considered according to separate sites earlier, notably by Ogle, but he stated that he did not think the expense and labour involved in doing so would justify the undertaking. At a later date this difficulty was overcome, and thus it is possible for Mr. Greenwood to-night to discuss in detail whether there has been an increase of cancer for certain parts of the body, or not. At the

beginnings of the investigations of the Imperial Cancer Research Fund the question had to be considered whether or not a cancer census should be instituted similar to that which had been taken in Germany. The conclusion was arrived at that this would fulfil no useful purpose and was an erroneous method. Instead it was decided to do what one could to encourage and, if possible, help the National Statistical Office in publishing more detailed data, particularly those referring to the incidence of cancer in different parts of the body. With this object, special investigations were undertaken as regards hospital statistics, not only in London but throughout the Empire. The early investigations into the occurrence of cancer in mankind speedily brought out the fact that the practices of different native races modified the anatomical distribution of the disease as known in Europeans. I am almost ashamed to repeat once more that some of these practices can almost be dignified with the title of unintentional experiments, notably the application of the Kangri to the skin of the abdomen in Kashmir, causing epithelioma to occur in a site from which it is absent in Europe and America. The chewing of betel-nut by the women in India makes them as liable to cancer of the floor of the mouth as men, a circumstance which does not obtain in Europe. The same may be said of the consumption of very hot rice by men in China leading to the development of epithelioma of the œsophagus, from which the women do not suffer, it is said, because they are served when the rice has already become cold. In animals, also, similar unintentional experiments obtain, the cow affording two instructive examples: the waggon is hitched to the right horn of draught cattle in India and epithelioma develops there and never at the left horn. In some parts of this country and Ireland cirrhosis of the liver is common in cattle, and on this cirrhosis there develops with great frequency adeno-carcinoma. The demonstration of these variations makes it necessary to admit the possibility of others, both in time and place. Observations on animals and experiments soon threw fresh light on the significance of the age-incidence of cancer, and the importance of considering the age-incidence for each organ separately.

I have not read Mr. Greenwood's paper with very great care, but I have marked one or two sentences on which I may be permitted to comment. It is said that general practitioners have no greater facilities for diagnosing cancer to-day than they had fifty years ago. I would point out that there has been a very great advance in the means of diagnosing other diseases, and as the diagnosis of cancer is still largely arrived at by a process of exclusion, this must unconsciously influence the general practitioner. There has also been a very large increase of beds in hospitals. I do not know if exact figures can be obtained for England, but they have been published for Germany, and there the increase has been enormous. This increase has coincided with the development of surgery, with the disappearance of the reluctance of surgeons to operate for cancer, and diminished fear of operations by patients themselves,

These facts also must have reacted upon the diagnosis of cancer by the general practitioner, quite apart from the general improvement in the accuracy with which all deaths are certified. I do not think that it is worth while discussing the increase of cancer upon the basis of the number of patients treated for it in hospitals. This is a matter into which I inquired some years ago. It was evident no estimate whatsoever was possible as to the population the cancer patients were drawn from and no basis for comparison could be obtained. The question of medical practitioners not being in a better position to diagnose cancer is raised a second time, and Mr. Greenwood inclines to the view that it is true that there are no improved facilities for diagnosing cancer. I do not agree with this statement in this bald form. It must be remembered that the way in which the term cancer is used to-day is very much more precise than it was fifty years ago or even thirty years ago. To take an example, I have here a paper by Waldeyer published in 1873. I select it because Waldeyer is the oldest and most distinguished investigator of cancer still living. He points out how cancer was originally purely a clinical term of vague meaning, and that a few decennia earlier the question of the real distinctions between cancer and other new growths had hardly been discussed apart from the superficial separation of certain external forms, such as scirrhus, medullary cancer, and fungus hæmatodes. Johannes Müller's definition was mainly clinical. Waldeyer quotes Hughes Bennett, who in the first book published on the histology of cancer in this country in 1849 wrote: "When we endeavour to define what a cancerous growth really is, according to the descriptions of morbid anatomists, or the symptoms of medical practitioners, we are at once thrown into a crowd of inconsistencies from which the sooner we emancipate ourselves the better."[1] I am familiar with the important literature from this time onwards, and it is quite easy to trace the progress by which our conceptions of cancer have been more clearly defined. This has been particularly due to the development of morbid anatomy and the greater frequency with which post-mortem examinations are made. Previous to accurate histological examination the relationship between cancer and its mother tissue had not been determined, and in this connexion it must be remembered that it was not till near the end of the seventies that the actual staining of tissues was introduced. The improvements of surgery have also contributed to our knowledge of the nature, origin, and spread of cancer, and they, too, have also been taking place within the last fifty and even thirty years. In 1884 the late Sir James Paget was unwilling to admit the truth of the statement that cancer was spread in the body by transportation of cells from the primary focus, and still maintained it was a constitutional disease due to some morbid product in the blood, although he

[1] In the preface Hughes Bennett writes: "No one can doubt that this disease is frequently confounded with epithelial, fibrous and other forms of growth, and that, up to the present time, practical men have no ideas sufficiently fixed and positive to govern their conduct in many important and dangerous cases."

had then sufficiently modified his views as to admit the possibility that this "morbid product" might be microbic.

The recorded increase in cancer is in marked contrast with the great diminution in the deaths attributed to tubercle and various other diseases, loosely classified as "wasting," "decline," and so on, and I note that in the literature, already referred to, much attention is devoted to the differential diagnosis of cancer from tubercular nodules: it may be that some of the diminution in tubercle is due to the transference of cases to cancer owing to more accurate diagnosis. As regards the age-groups among which the increase is recorded, it may be worth while pointing out that ten years ago the belief was held in Germany by distinguished persons that the most alarming feature of the increase of cancer was its increasing frequency in younger age-groups. This conclusion was not then in accordance with the English vital statistics, and quite recently I notice that figures have been published for Germany showing that in that country also the increase is most marked in higher age-groups.

As regards Mr. Greenwood's curves, there is only one to which I would like to make reference. If I understand Mr. Greenwood rightly, he draws a contrast between London and Somerset in the following way: If improved diagnosis were a factor responsible for the increase in the number of deaths recorded from cancer, then the rate of increase should be greater for Somerset than for London, because it lags behind London. In other words, the curve for Somerset should be steep and that for London should show a flattening on account of the operation over a longer period of the greater facilities enjoyed in the metropolis. But we are entirely ignorant of the absolute incidence of cancer, and therefore should like to ask Mr. Greenwood whether he has considered the possibility that—owing, as it were, to there being a great deal of slack to take up in London—the greater facilities obtaining in London might still cause a more rapid improvement in London than in Somerset. When considering institutional deaths, Mr. Greenwood leaves out workhouse infirmaries and workhouses, but it has been my experience that there are workhouse infirmaries and workhouses in which a large number of deaths do occur from cancer and come to autopsy.

The increase in mammary cancer contrasted with the standstill or actual fall in deaths from uterine cancer raises a very important point. We do not know what the absolute incidence of cancer is. For those sites of the body which are very easily accessible to complete examination—e.g., the skin and uterus—and which do not show any incease or actually show a fall, it may be that we have approximated very nearly to the absolute incidence of the disease, and that, therefore, the effects of successful operation are beginning to show themselves. While mammary cancer is much more difficult to diagnose than uterine cancer, and is certainly diagnosed more frequently at a later stage than is uterine cancer, it is difficult to express an opinion whether an increase is or is not taking place. The matter has been discussed from the

standpoint that the diminution in the number of childbirths may favourably affect the fall in the uterine rate, whereas the inability or unwillingness of women to suckle their children may not be unassociated with an increase of mammary cancer. It is worth while pointing out that in 1906, in a paper I read before this Society, I showed for three different age-groups that cancer of the uterus had been for a number of years diminishing in Berlin. It would therefore appear that for the uterus at any rate the phenomenon is one of general occurrence. In the case of the increase of cancer of the tongue it must be pointed out that, in contrast to the uterus, operation is here conducted under very great disadvantages. With the late Sir Henry Butlin, Murray and myself investigated very carefully fifty-six of the earliest epitheliomata which it was possible for us to obtain. Although some of these growths were no more than 2 or 3 mm. in diameter, and the lymphatic glands were removed, the results of the operation were often unfavourable. Owing to the unfortunate premature death of Sir Henry Butlin I am not in a position to give the exact results, but, speaking from memory, my impression is that in less than half of the cases was recurrence absent and patients still alive and well at the time of Sir Henry's death.

On the whole Mr. Greenwood's paper supports the views I have expressed that the increase recorded in the number of deaths from cancer is not uniform for all parts of the body, and that it is wrong to speak of an increase of cancer as a whole. It bears out, further, the conclusion that the problem can only be solved by considering each site separately. Some years ago this question was raised in connexion with the occurrence of cancer in different occupations, and owing to financial questions involved, the Imperial Cancer Research Fund undertook to provide money in order to have the data which existed in the Registrar-General's office abstracted. Subsequently it was found possible to provide the financial necessities from other sources. Mr. Greenwood raises the question of the value of tabulating the deaths from cancer for the several sites in separate administrative divisions. I have already drawn attention to this point. Dr. Stevenson has just stated that the data for different parts of the body are available for separate areas, although he does not consider the present time is favourable for having them tabulated. When it becomes advisable to have these additional data abstracted, I should deem it a question worthy of raising before the Executive Committee of the Imperial Cancer Research Fund whether financial assistance should be given, if required. On the whole, I am inclined to think that some of the problems connected with the increase in the number of deaths recorded from cancer come before us in a form in which we really cannot solve them, because we do not know what the absolute incidence of cancer is, and we cannot get hold of the primary data we require for the human subject with sufficient accuracy. Hence for many years breeding experiments have been conducted in our laboratories by Dr. Murray. He has now bred some 3,000 mice, and has collected very accurate data regarding the absolute incidence of cancer in

these animals as regards different parts of body, age, and the influence of heredity. Having got data as to the absolute incidence of the disease it becomes possible to study what factors modify its frequency, increasing or diminishing it — e.g., chronic irritation. It may be years before we have definite information on this subject, but much may be attained before some of the problems we are discussing in regard to mankind have advanced from the position in which they now are.

Before sitting down it would be unfair of me were I not to pay a tribute to the willingness with which Dr. Tatham and Dr. Stevenson have assisted the investigations of the Imperial Cancer Research Fund.

Dr. Horst Oertel: Allow me first to thank you for having given me the opportunity to listen to, and profit by, this interesting and instructive paper with its illuminating discussion. I speak with some feeling in this matter, because I come, as you know, from a country in which the scientific treatment of medical statistics is, on account of political, administrative, and educational difficulties, which have only lately been justly criticized by Dr. Bashford, still in its infancy. England, on the other hand, has made great advances in this branch of statistics, and is to-day a well-recognized leader in this movement. The preceding discussion has left very little unsaid, and I could add nothing of value to the statistical questions, but I may perhaps be allowed to say a few words about the diagnosis of cancer. This, it seems to me, is a point which must not be overlooked or taken too lightly in the treatment of cancer statistics. I agree with those who believe that there has been an improvement in the diagnosis of cancer during the last twenty-five years, perhaps not in so far as the opportunities of the general practitioner to verify his opinion by autopsy are concerned, but more especially in the better crystallization of our conceptions of cancer; twenty-five years ago these were, at least in the mind of the general practitioner, more hazy than to-day, for pathologists had only laid down our modern conception of cancer a short time before. But there is perhaps to-day a tendency to diagnose cancer a little too frequently, and cancer shares in this respect the fate of certain other diseases when brought to the immediate focus of the public eye. At any rate, I have noticed that during an epidemic, when attention is centred on a disease, an almost hypnotic influence seems to prompt a diagnosis when it would under ordinary circumstances not have been made. However, I have also observed the very opposite, for physicians sometimes hesitate to certify to certain diseases like cancer, tuberculosis, syphilis, and diabetes, on account of regard for family or other more personal reasons. I rather think that this is decreasing, but I believe that it entered at one time, and not very long ago, into the errors of vital statistics. Finally, it must be remembered that the diagnosis of cancer is really a most difficult one, that it requires careful training in pathological anatomy, and that frequently mistakes are made even at autopsy which are cleared up only by subsequent painstaking microscopical examination. It is, of

course, impossible to say whether these various sources of diagnostic error may balance each other in the end. Permit me to say in conclusion that whatever inaccuracies and deficiencies may attach to the present methods of vital statistics, the grave consideration which they have received of late by such societies as yours has aroused the majority of the medical profession to their responsibility in the matter, a responsibility to which only too few devoted serious attention in the past.

Dr. FARRAR pointed out that the improvement in surgical technique which had taken place during the last generation must have saved a very considerable number of lives among cancerous persons who after the removal of cancerous growths survived to die ultimately of some different disease. He specially instanced, and illustrated from his own experience as a general practitioner, cancerous growths of the uterus, tongue, and mammæ. Only a generation ago uterine cancer was regarded by leading surgeons as "inoperable." The saving of life by operations on such cases, while not reducing the real incidence of cancer, must certainly tend to reduce the apparent death-rate from this disease, and would, to some extent, counterbalance the apparently increased incidence due to improved means of diagnosis.

Dr. DAVID HERON: I should like first of all to call attention to the fact that in Tables IA and IB the sexes have been grouped together, although it is now generally recognized that in any discussion of disease rates they should be dealt with separately. If we look, for instance, at Table IIB, we see that the male cancer death-rate in London in 1901 was 864 per million, while the female death-rate was 1,092 per million. Further, while the male rate increased in ten years to 1,058, the female rate showed a slight decrease. There is thus considerable danger in averaging rates which differ so markedly. In the second place, it is axiomatic in modern statistical work that whenever we sum up any of the features of a series of observations in a single number, we should always make some statement as to the weight that is to be attached to such a constant. An average, or a correlation coefficient, should always be accompanied by a probable error in order that we might know the probability that any value of the constant concerned might arise from pure chance. The constant which is used throughout this paper is a "gradient," actually the slope of the best fitting straight line to the index numbers of the various series of death-rates, and unfortunately the authors do not give any indication of the relative significance of the differences between the various gradients on which their conclusions are based, although these gradients are based upon short series of 10, 15, or at most 20 death-rates, all of which are subject to errors of random sampling as well as to changes in local conditions. To take an example: In dealing with Table IIB, the authors state on p. 18 that they "undoubtedly find a somewhat different state of affairs, since now there is a general distinction between the urban counties on the one hand and the

rural on the other. For males, all the urban lines show a less steep gradient than do the rural ones, precisely as we should anticipate on the simple hypothesis, and there is a similar distinction in the case of females." Turning now to Table IIB, and confining our attention to male deaths, we find that the lowest gradient among the urban counties is 1·273 in the case of Northumberland, while the steepest gradient among the rural counties is 2·030 in the case of Somerset, the assumption being that there is a significant difference between these two gradients. I have endeavoured to illustrate these gradients in diagram form in fig. 1. The continuous lines give the best fitting lines in each case, while the dotted lines give those with which they are to be

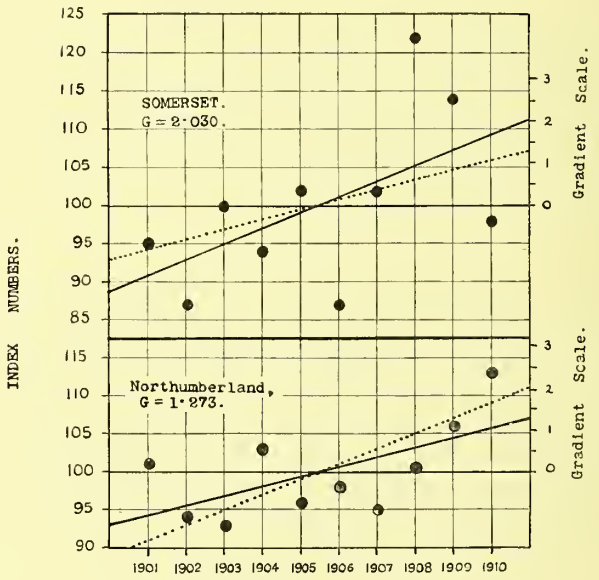


FIG. 1.

Index numbers of the corrected death-rates from cancer in Somerset and Northumberland with (*a*) the best fitting straight lines (continuous lines) and (*b*) the lines with which they are to be compared (dotted lines). Males, 1901-10, Table IIB.

compared. Now the authors' contention is that the gradients in those two cases are significantly different, but in each case the dotted line appears to give practically as good a fit as the continuous—i.e., as the best fitting straight line. If we measure goodness of fit by the root mean square deviation, we find that in the case of Somerset this constant is 8·65 and in the case of Northumberland 4·60, and any line with a slope between −1·2 and +3·8 will give a better fit to the Northumberland data than the best fitting straight line

to the Somerset data. In such circumstances comparisons of the gradients of these lines should be made with the greatest caution. What is needful is some measure of the weight to be attached to the gradient in each case, and until this has been supplied we cannot tell whether the differences between the gradients are significant or not. The authors tacitly admit the need for some measure of this kind, for they leave out from Table IB the figures relating to Wiltshire, "the results for which in the case of crude rates seemed to argue that the population at risk was too small to afford reliable indications" (p. 15).

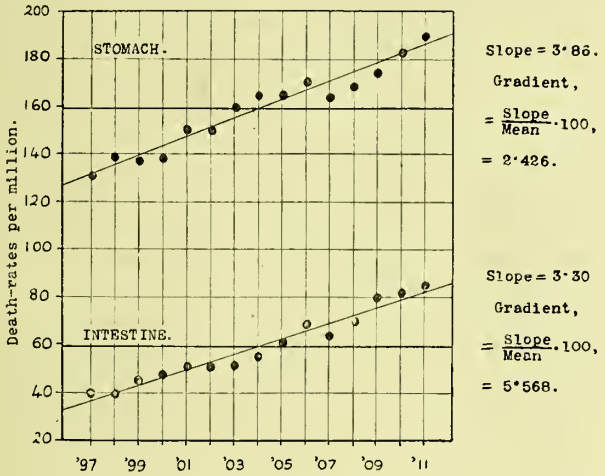


FIG. 2.

Crude death-rates from cancer of stomach and intestine with the best fitting straight lines. Males, 1897-1911, Table VIA.

If we examine the constant used throughout the paper, the gradient, we find that it is actually—

$$\frac{\text{Average Annual Rate of Increase of Cancer Death-rate}^{1}}{\text{Average Cancer Death-rate of the Period.}}$$

The gradient is thus made up of two factors which may vary independently, and even if the annual rate of increase remained constant, a difference between the average death-rates in two areas or sites would cause a difference between the gradients. Thus in Table VIA it is stated that in cancer of the stomach in males the gradient is 2·426, while in cancer of the intestine the gradient is 5·568, but if we examine the actual death-rate as shown in fig. 2 we find

[1] Deduced from the slope of the best fitting straight line to the death-rates.

that the slopes of the best fitting straight lines are much the same (3·86 and 3·30), but since the average values are widely different (159 compared with 59) the gradients are also widely different. The differences between the gradients thus appear to be very largely due to differences between the average values of the death-rates, and it is these averages which the authors are really comparing if the slopes of the best fitting lines to the death-rates are nearly the same. I think, therefore, that the absence of any method of testing the significance of the differences between the gradients and the very grave doubt as to the propriety of using the gradients of these index numbers at all should make us unwilling to accept, without much more investigation, the authors' conclusions. Only when the statistician starts with a thorough mathematical training will it be possible, as the authors of this paper suggest, to avoid the fallacies which result from the "unskilled treatment of medical statistics."

Dr. DUDFIELD said he had read the paper before the meeting with much interest, but he had to admit that he did not possess the mathematical knowledge required to recognize the omissions and errors on which Dr. Heron had animadverted so severely. He (Dr. Dudfield), while holding the view that there had been a real increase in the mortality from cancer, thought that some of the recorded increase was probably the effect of changes in classification, a view which he thought was supported by the sudden increases in rates given in Table IB about the years 1889 and 1890. A similar explanation had been adduced with reference to the enormous increase in the recorded mortality from injury at birth to which he had recently called attention in the *Journal of the Royal Statistical Society*. The practice of making special inquiries with reference to certificates of causes of death had admittedly brought out, in a considerable number of cases, the fact of death being due to cancer, although that cause was not mentioned in the original certificate. That practice was undoubtedly more general now than formerly, and might be considered to be a further cause productive of some of the increase. His own experience with regard to such inquiries—not by any means limited to cancer—had convinced him of the urgent necessity of making the certificate of cause of death a confidential document—not accessible either to the relatives of the deceased or to insurance offices. He adverted to the Swiss practice in this respect, but hoped that it would not be found necessary to follow that practice absolutely and so deprive local medical officers of health of the information they now obtained. He thought the certificates should be sent to the medical officers of health. The authors had referred to the supposed superiority of death certification in hospitals and other institutions resulting from post-mortem examinations. His experience was that the death certificate was generally issued prior to the making of such an examination. He had not infrequently ascertained that a post-mortem examination had disclosed a very different cause of death to that stated in the official certificate, but, so far as he knew, no steps were taken to amend the certificate.

There was one aspect of the subject to which the authors had not alluded to—viz., to what extent was the increase in the expectation of life responsible for the increase in cancer? As death inevitably followed birth, it was simply a question as to which of the many trap-doors seen in the Bridge of Life described in the Vision of Mirzah became the channel from life to death. If those in the arches of youth and adolescence were safely passed over, those to be found in the arches of old age (among which was the trap-door labelled "cancer") snatched the survivors. The subject of the evening's discussion had been the *mortality* from cancer. That he thought had increased, but had there been a corresponding increase in the *prevalence* of cancer? In his opinion there were no data at present on which to found an answer to that question. The only possible source from which the necessary information could be obtained lay in the statistics of hospitals and other institutions. In the Report of the Committee on Morbidity and Mortality Statistics issued last year by the Royal Statistical Society attention had been directed to the absence of any serious attempt, except at two or three of the largest hospitals, to make use of the very valuable statistical data available. It appeared to him that there was an urgent need of a central statistical organization to collate such statistics, and he had some hope that at no distant date a scheme would be formulated in that direction. As early as 1840 attention had been called to this subject, but realization was still to come. That the statistics when properly collated ought to yield much instruction was, he thought, obvious. The size of the sample—some 120,000 in-patients passed through the London hospitals each year—was sufficient to furnish results fairly free from errors of sampling.

Mr. MAJOR GREENWOOD, jun., and Mrs. FRANCES WOOD (reply): The hope that our paper would initiate a good discussion has been fulfilled, and we desire cordially to thank all who have taken part in it. We think the President has attributed to us a somewhat more dogmatic attitude on the subject of diagnosis than our actual words imply. We merely say, "We do not think that the progress of research has placed in the hands of the ordinary practitioner direct means of diagnosing cancer not available even so long as fifty years ago." We are sorry if the wording of our paper seems to attribute to Dr. Stevenson a more decided opinion respecting the cause of the change in the recorded cancer rates than he holds. We did not intend to convey such an idea and merely singled out his hypothetical question because it crystallized a view which seemed to us peculiarly intelligible and useful as a working hypothesis. We tender our apologies to Dr. Stevenson for our unintended misrepresentation and have inserted an explanatory footnote to the text.

Dr. Bashford's speech was heard by everyone with the greatest interest and we have learned much from it. His masterly exposition of the reasons which made it desirable to abandon the discussion of cancer rates *en masse* and

almost imperative to confine attention to site-rates amply justifies the doubts expressed in the concluding portion of our paper. With regard to the question he asked, our point was that if improvements of means for effecting a diagnosis had been generally operative, we should have expected the more backward counties to improve faster than the better educated ones, just as in many branches of education progress is rapid up to a certain point and then becomes slower. We agree, however, that such a rule may be subject to numerous exceptions.

Dr. David Heron's criticisms fall under three heads: (1) We have not computed the "probable errors" of the constants of the straight lines applied to the index numbers. (2) We fitted lines to index numbers instead of to rates. (3) In one section an argument is based upon rates in terms of persons. Dr. Heron appears to us to attach to the term "probable error" a meaning which the majority of statisticians would repudiate. If we take a random sample of adult Englishmen, measure their heights, and say that the average stature is a, with a "probable error" of $\pm b$, we imply that we should not be surprised were other samples to exhibit mean values which diverge from that of the first within limits assigned by the magnitude of the "probable error," and that we are only entitled to infer that the "real" mean of the whole population falls within certain assignable limits. Had we measured the whole population of living adult Englishmen, the average value obtained would have been an exact quantity, within the limits of errors of measurement which are not, of course, defined by the computed "probable error," and the "probable error" of the mean would have been zero. This is precisely the state of affairs in the instance of our site-rates, the data are the true *recorded* proportions in the whole population during the period under examination; the series of values is not random, and does not consist of samples, hence the question of "probable errors of random sampling" does not arise. The case is evidently analogous to that studied by Professor Karl Pearson in his pamphlet, "The Fight against Tuberculosis and the Death-rate from Phthisis" (Dulau and Co.). Professor Pearson published three diagrams: (*a*) The general death-rate of England and Wales; (*b*) the phthisis death-rate; (*c*) the ratio of phthisis deaths to all deaths. The original figures seem to have been the crude rate for males and females separately from 1835 onwards. Each series is divided into approximately the same periods—viz., 1835-66, 1866-91, and 1891-1910, and "with the kind help of Miss Ethel Elderton, regression straight lines have been fitted to the graphs" of these periods. Various important deductions are made from the varying slopes of these lines, some of which, so far as we have been able to test them, do not fit the observations better than some we have employed in our own problem. Professor Pearson, like ourselves, appears to have been under the impression that the theory of random sampling had no direct bearing upon the problem he desired to solve, and his offence in Dr. Heron's eyes must have been more heinous than ours, since he was addressing a popular audience unlikely to realize the

enormity of his crime. It may be said that this argument does not apply to the comparison of one county with another, since, *if* the rural counties formed a homogeneous "population," any one of them might be regarded as a sample of the whole. In the first place, the counties are not homogeneous, and in the second, if they were, it would be best to avoid risk of error by confining the comparison to the aggregate of rural counties on the one hand and urban counties on the other: the course adopted in Table III and Diagram III. In view of the non-homogeneity of the counties, the comparison of individual cases is probably more instructive. It will be found that Professor Pearson in his pamphlet, "The Problem of Practical Eugenics," has also used this method of lines in comparing diagrammatically the changes in the birth-rate during different periods in counties and towns, except that he has employed rates not index numbers, and has not published the constants upon which the lines which appear in his diagrams are based. Had we asserted that our results were applicable to an indefinitely large "population" of counties returning rates over an indefinitely large number of years, then it would have been relevant to point out that constants based upon ten or twenty years' records from a handful of counties were not, unless we could determine something analogous to a "probable error," a sound basis for generalization. Of course, we made no such assertion, and we think Dr. Heron has forgotten the truism that a result which is exact for one purpose is inexact when used for some totally distinct purpose. We wonder why Dr. Heron did not ask himself the reason for our computing "probable errors" in the case of the Swiss material alone. The point was that, in that case, we *did* attempt to generalize our results. We wished to know how far a value obtained from a series of districts which we assumed, very arbitrarily we admit, to be a sample of an indefinitely large "population" of similar districts might be trusted as a general measure of certain associations. In the other cases we had no such object in view, we were not concerned with the experience of London from, say, 1881 to 1900, as a sample of an indefinite number of years, but with the relative changes within that specific period. It appears to us that problems of the present class are essentially problems of graduation. We are given a series of values, *a*, *b*, *c*, *d*, &c., and the problem is to graduate them; the theory of "probable errors" concerns itself with the totally different problem—viz., how far the series to be graduated, assumed to be exactly observed, adequately represents an experience of which it is a sample. The inquiry presupposes that the series is but a sample of a wider series, and it is difficult in the case of the counties, impossible in that of the whole kingdom, satisfactorily to define such a wider series. To use the ordinary notation of "probable errors" in such a case would be as reasonable as—perhaps less reasonable than—to insist that the expectation of life at a given age assigned by the London Life Table must not be compared with that shown for the same age in the Brighton Life Table, unless the "probable errors" of the values are computed.

We may add that the ordinary method of computing "probable errors,"

even in cases where their introduction is appropriate, sometimes, in our opinion, may tempt the reader to neglect other sources of error. This is due to the fact that if we have a series of n rates calculated on populations each of m, the ordinary method of "probable errors" pays attention to n, not to m. Thus, in Dr. Heron's valuable paper on the correlations of the birth-rates in London boroughs with other rates, the "probable errors" of his coefficients are computed on the assumption that the smallest borough yields a rate equally valid as an observation with that derived from the largest. If we only regard such "probable errors," it follows that, e.g., a correlation of 0·5, deduced from a sample of forty cities with populations between 20,000 and 50,000, is more reliable as a basis for generalizations than the same correlation deduced from twenty cities with populations between 100,000 and 200,000, an inference which, in the case of disease rates, may be quite unwarranted. A consciousness of this difficulty makes most observers hesitate to use rates deduced from small absolute numbers,[1] or to include in the same series rates derived from very unequal populations.

Turning to the question of goodness of fit—the effectiveness of the graduation by straight lines—we are perfectly in agreement with the view that some of our lines are but poor fits, although we think that a similar remark applies to certain of the lines upon which Professor Pearson based more definite conclusions than are to be found in our paper. Indeed, we endeavoured to make our opinion clear that the comparison between urban and rural counties did not, on the whole, establish any marked difference in rate of change, hence the negative answer to Dr. Stevenson's hypothetical question.

Passing to the question of index numbers, we think the method adopted to be that which best secures the end in view—viz., the provision of comparable results. It seems to us that the significance of any given increase largely depends upon the relation such increment bears to the average magnitude within the series. This consideration has influenced the very large number of statisticians who employ index numbers for comparative purposes. It is, of course, possible to attain the same end in other ways. Thus Professor Pearson, in the pamphlet already mentioned, while using actual death-rates changed the scale of the diagram when he passed from general to phthisis death-rates. The vertical scale of the diagram, showing the best fitting lines applied to phthisis rates, was ten times as large as that used in the illustration of general death-rates, a device which does not attain the desired

[1] If the population upon which a rate is calculated be regarded as a sample, it is, of course, easy to determine the standard deviation of sampling of the rate or, by an application of Bayes's theorem, to ascertain the probability of two populations with different rates standing one to another in the relation of first and second samples from a common "universe." We cannot, however, by either method, gauge the significance of possible errors in classification which may more seriously vitiate the returns from a small population than those from a large one. Hence, altogether apart from questions of sampling, it is necessary to look with some suspicion upon rates derived from small populations. This was one reason for omitting Wiltshire from one of our comparisons.

end any better than ours. The method of presentation must vary with the particular aspect of the problem it is desired to study; we were interested in relative changes, not absolute differences, used the method appropriate to the treatment of the former, and do not share Dr. Heron's "grave doubt" as to its propriety.

Lastly, we refer to the criticism of our use of rates in terms of persons in one section. Since we provided figures corrected for variations in both the age and sex distributions of the populations under discussion, and only used the figures to try to answer Dr. Stevenson's hypothetical question, the objection seems of little importance. If diagnosis is better in London than in Lincolnshire, it is better in the case of both males and females.

We may add that even when the problem under discussion does seem to require the separation of the sexes, such rates, if uncorrected for age, may lead one further astray than the corrected rate on persons. Thus, in his memoir on the "Relation of Fertility to Social Status" (Dulau and Co.), Dr. Heron writes (p. 12): "We come now to consider the relation between the birth-rate and the prevalence of cancer, and here we get a somewhat unexpected result. Taking both sexes together, we find: Correlation between the birth-rate and death-rate from cancer = $-0{\cdot}563 \pm 0{\cdot}089$; in other words, the district with a low birth-rate suffers most from cancer." He then proceeds to work out a series of correlations between the cancer rates for males and females and a variety of measures of social status, concluding that there is a significant positive association between the prevalence of cancer and conditions of good social status. Had Dr. Heron simply used the age and sex corrected rates published by the Medical Officer of Health for the County of London, he would, we think, have doubted whether his result might not mainly be due to the more favourable age constitution of the richer districts from the standpoint of cancer incidence. If the corrected rates on persons are used, the correlation shrinks to $-0{\cdot}002 \pm 0{\cdot}132$.[1] A similar reduction is effected if for crude female rates we substitute the age corrected values, Dr. Heron's correlation of $-0{\cdot}535 \pm 0{\cdot}093$ becoming $-0{\cdot}177 \pm 0{\cdot}126$. In fact, the neglect of age standardization seems to us a more serious source of error than the use of persons even in such a problem as that dealt with by Dr. Heron. In our own case, for the reasons given, we do not regard the matter as of importance.

Dr. Oertel's remarks were very interesting, particularly his observations on the difficulties of diagnosis, and we are not unmindful of the questions raised by Dr. Farrar in connexion with the results of surgical treatment.

[1] This was obtained after omitting Deptford and Greenwich, which Dr. Heron grouped together, but which are separated in the Medical Officer of Health's table of corrected rates. The value obtained from Dr. Heron's data after omitting the combined district is $-0{\cdot}555 \pm 0{\cdot}092$. The values for corrected rates are quoted from a forthcoming paper by Brown and Lal, who have re-investigated the subject and can find little, if any, correlation between measures of social status and cancer death-rates in the London boroughs, a result in full agreement with the view expressed in the Annual Reports of the Public Health Department.

The difficulty is to obtain trustworthy statistical data as to the effects of operation. This can be well seen in Dr. Lewers's treatise on cancer of the uterus.

We agree with Dr. Dudfield that the time is ripe for a proper utilization of hospital statistics, although, for the reasons adduced by Dr. Bashford, we doubt whether even the most accurate hospital statistics will throw a great deal of light upon the problems discussed in our paper.

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